SUPPLEMENT

TO THE FOREGOING

LECTURES

ON

COMPARATIVE ANATOMY.

ILLUSTRATED BY ENGRAVINGS.

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PREFACE.

In bringing before the public these two supplementary volumes on Comparative Anatomy, I have communicated all the materials of any importance, which I have been able to collect, during a longer life spent in this pursuit than most men engaged in scientific investigations have enjoyed.

The six volumes, taken together, are filled with researches in Comparative Anatomy, that were begun at seventeen, and have been uninterruptedly continued to seventy-two,—a period of fifty-five years.

It is to the late John Hunter I owe the love of the pursuit, and his example both of application and the mode of investigation I have never lost sight of. To him I am indebted for the rich stores which he placed before me, which it has been the height of my ambition to increase; and my life has been continued beyond what is considered the usual period allotted to man, with the

possession of my faculties sufficiently perfect to carry on my pursuits. Early habits have made me prefer the entertainment resulting from the exposition of the beauties of nature, and the delight to be derived from examining the works of the Creator, in which infinite wisdom and intelligence are displayed, to all other amusements.

I am now enabled to connect together the separate investigations contained in these volumes, by means of a general explanatory index, in two parts; one of the letter-press, the other of the plates.

In this way the materials of the different lectures will be brought into one regular course, so far as reference is concerned; and the plates will, by such an index, form one regular series, in which every representation will be brought into its proper place.

ERRATA IN VOL. V.

Page 192. line 3. *for* containing *read* contained in.
249. lines 4. and 6. *for* lama *read* al paca.

DIRECTIONS FOR THE BINDER.

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INTRODUCTION.

So minute are the materials of which animal bodies are composed, that they do not come within the reach of Anatomical Observations unaided by the use of the microscope; and, therefore, however great the advances that have been made in anatomical researches, very small have been the approaches towards a knowledge of the more minute structures.

Within these last fifteen years, during which Mr. Bauer's microscopical labours have been applied to Comparative Anatomy, so many new lights have been thrown upon different parts of the subject, and so many new discoveries made, that I consider the period to have at last arrived, in which we can attempt an enquiry into the materials out of which the different organs of an animal are composed.

All first attempts must fall very short of their object; if, however, in so very important an investigation, by bringing together the collective evidence, the result of Mr. Bauer's labours for so many years, we shall be able to trace the different steps by means of which the food is converted into

blood ; afterwards, to analyse that fluid and separate the parts from one another, pointing out such as are employed in the formation of the brain, the great regulator of all the actions that take place in the animal economy, and those from which muscular fibres are derived, we shall have made no small addition to our stock of knowledge in Comparative Anatomy.

This, I trust, I shall be able to do by the present additions, when they are made a part of the same work with the volumes already published.

This work would have fallen short of its value, as well as of its use to science, had I not been indulged by the President and Council of the Royal Society with the use of the plates taken from Mr. Bauer's drawings that have been engraved at the expence of that learned body, and are registered in the Philosophical Transactions.

CHAPTER I.

On Digestion, and Concretions formed in the Stomach.

Galen, when studying human anatomy, was so struck with the perfection by which all the parts of the human arm and hand are adapted to one another, that he composed a hymn to the Deity, expressing his admiration of a piece of so much excellence.

Were I, as an enthusiast in Comparative Anatomy, to illustrate the wisdom and the forethought of the Creator, from the mechanism of any one organ met with in animals, it would be the organ of digestion, as affording me the largest field for displaying the wonderful superiority of the works of nature over those of art.

Having, in the first and third volumes, given in detail the various structures met with in the stomachs in different classes of animals, the only new facts I have since acquired respect the Walrus.

SECT. I.

The Organs of Digestion of the Walrus.

This animal, an inhabitant of the arctic regions, has had its natural history made known to us by the late expeditions towards the North Pole. Among other things, I am informed by my friend, Mr. Fisher, who was astronomer and chaplain to Captain Parry's ship during his two first voyages, that the food of the walrus is a species of sea-weed that has hitherto been considered as not digestible by the animals that inhabit those seas; but we have now undoubted evidence of the organs of digestion in the walrus, which in some respects differ in their organization from those of any other animal I ever examined, being fitted for this purpose; and this less from a peculiarity in the stomach itself, than in the mechanism interposed between it and the liver, to enable this animal to digest a plant, by which it is destined by nature to be nourished. It is the fucus digitatus, a vegetable production, very abundant in the arctic seas, in which the walrus is met with in great numbers.

Mr. Fisher, fortunately for this enquiry, was present in the examination of the stomach of a walrus which they had killed; and so disgusting were its contents to one of the common seamen, consisting of the half-digested state of this fucus, that it made him sick. I need not mention the effects it produced on the rest of the party present.

A supply of this fucus as food for the walrus can never be wanting; since, when the sea is open, it is thrown upon the beach in great abundance, and when the sea is frozen, is met with in masses on the ice.

The stomach of the walrus is, in its internal membrane, nearly allied to that of the seal; but, to adapt it to this species of food, the gall-bladder is unlike every thing met with in the seal, manatee, and dugong, proving that there must be a peculiar process required for the digesting of this fucus, which is, probably, the principal nutriment of the walrus, and only occasionally that of the seal; for both in the seal and walrus the œsophagus admits of regurgitation of the undigested part of the food.

In the seal, the liver and gall-bladder do not materially differ from those of other animals.* In the walrus, a large cylindrical hard body lies behind the duodenum, loosely connected to it by a cellular membrane; at its lower end it projects into the duodenum like an os tineæ. This is not the common opening of the ductus communis collidicus, but a canal leading directly from the gall-bladder, which is a large oval cavity, with strong thick coats, capable of ejecting its contents with considerable force. The bile is brought from the liver into this reservoir, by a single small duct, which enters at a lateral opening. This mode of conveying bile into the duodenum is like nothing that we have been hitherto acquainted with; the parts are represented in the drawings.

* The gall-bladder is small, detached from the liver, and opens by a small orifice, two inches and a half from the pylorus, into the duodenum. The orifice at the pylorus of the stomach is extremely small and valvular.

The mucus secreted by the coats of the gall-bladder, which in other animals is small in quantity, in the walrus is so abundant as to appear to answer some particular purpose when mixed with the bile. From the very contracted state of the pyloric orifice of the stomach, and its valvular form, the bile cannot readily be forced into the stomach; but, probably, whenever the digested fucus arrives in the duodenum, the necessary quantity of bile is ejected from the reservoir and mixed with it. The various purposes the bile is to answer, may be ascertained by future philosophers, but they are, at present, only to be guessed at. No doubt can be entertained, that the formation of fat in the intestine is one of them.

The preceding facts, and those I have already published, may be considered a sufficient body of evidence in proof of fat being formed from the contents of the intestinal canal; but, having had another instance in proof of this in the human body, I shall add it in this place; since new opinions may be rejected from having too few facts brought forward in their support, but cannot well be supported by too many.

A man thirty-six years of age, a patient of my friend Dr. Babington, felt an uneasiness in the back, for which he took the blue pill for two or three months. This relieved him, but the pain did not go wholly off. Upon paying attention to what passed off by the bowels, he found that before the fæces came away, a yellowish semifluid matter preceded them, with a remarkable smell, and followed by scybala streaked with whitish films; and it was not till after these had been expelled, that any regularly formed fæces passed, the inferior portion of which was somewhat in-

durated, while the upper was soft, the colour lighter than in health.

At present, the person feels pain in the lower bowels, and the stools are accompanied by flatulency; there is no pain in the liver or other viscera.

There is no disposition to nausea, the appetite good, the sleep sound; sometimes in dreaming the same inclination is felt to go to stool as when awake.

The most distressing circumstance is the involuntary passing of the oily matter just mentioned. With alkalies it forms soap; burns with a clear light, when moulded round a wick: it resembles the butter poured over potted meat. The quantity voided in twenty-four hours is a wine glass full. The patient has avoided all greasy food; but this does not prevent its formation, although he thinks that the quantity is increased by their use. The most curious circumstance respecting fat is, that, except by the intestinal canal, it has never been detected passing out of the body; and, in the remarks which I shall have occasion to make hereafter, it will be found, that oil not only does not escape in the urine, but that there is a mechanism employed to prevent the quantity in the circulation exceeding certain limits, and, at the same time, to give any surplus that may have arrived at the renal arteries, a ready conveyance back to the thoracic duct by means of absorbent vessels.

The ambergrease met with in the intestine of the whale is a species of fat in the form of scybala, very strongly impregnated with essential oil, and, I believe, only occurs when the animal is out of health.

SECT. II.

Digestion under the Influence of the Nerves.

Hunger is induced by the state of mind brought on by the impression carried to the sensorium from the nerves belonging to that portion of the œsophagus in which the gastric glands are situated; and it is not improbable that the secretion of that juice into the cavity of the stomach, when there is nothing for it to act upon but the mucus with which the internal surface is lined, may bring on irritability of that internal membrane, producing faintness, and sensations of the most distressing kind.

In this state of hunger, the pleasurable feelings from food being received into the stomach, are highly gratifying, and the taste which is left upon the tongue from the first glass of wine, under these circumstances, is that of much higher flavour than any subsequent glass of the same wine during that meal can produce.

When the hunger is extreme, the cavity of the stomach becomes distended by air, the motion of which increases the distress; this is relieved by external pressure, as is well known to those who have been long kept without food.

If any one in this state of hunger eats ravenously, or takes one morsel in quick succession after another, it is apt to produce a spasmodic contraction of those fibres that form the temporary partition between the cardiac and pyloric

cavities while digestion is going on; and when this happens, the pain that follows instantaneously is so great as to make the person faint away, and remain in that state for a considerable time. This is probably produced by too sudden a displacement of the air from the cardiac cavity. A glass of wine will take it off, if the person has presence of mind to take one the moment the feel of distress begins, for afterwards he has no power to do so. This feeling has very frequently occurred to myself, and I have always prevented the consequences, when wine was within my reach; but I had lost all power of asking for it. As soon as the cardiac cavity has been moderately distended with food, the stomach is brought into a tranquil state, and no such disturbance is likely to occur.

When very hungry, the thirst excited is often very great, and the desire for drink greatly exceeds what is required to form the compost of a proper consistence for the process of digestion in the pyloric cavity. In many individuals large potations are absolutely necessary to quench this thirst, whatever the liquid may be. The purpose answered by these fluids I shall endeavour to explain. As soon as the valvular contraction prevents more liquids from passing, whatever is drank afterwards, by distending the cardiac cavity, stimulates the absorbents which I have shown its coats to possess; and these fluids are carried into the trunk of the vas breve, and from thence by the splenic vein through the vena portæ, and from the liver into the general circulation, diluting the serum in a very great degree, and carrying off the impurities which the serum had dissolved in it, or were mechanically mixed with it: of the first kind are the salts in

the blood; of the second the essential oils taken by the mouth, which pass out directly from the cardiac portion of the stomach.

As the essential oils never reach the mass in which digestion is going on, so the alcohol in all fermented liquors is never carried into the absorbents of the stomach to be conveyed into the circulation, but passes by the pylorus. The internal membrane of the cardiac portion is so organized, as to secrete a large quantity of a very viscid mucus; the membrane of the pyloric portion is very different; and the membrane of the duodenum still more so. These various secretions must all be more or less combined with gastric juice, and make a part of the compost that is to be the subject of digestion; add to which it would seem that the terminal arteries throw out their contents into the cardiac cavity, since, in the examination of its secretions, blood globules are met with, so that one part of this compost is a part of the blood ready formed. From all these circumstances, digestion is a more complex and delicate process than it at first appears to be; and to render it complete, a great deal of trituration is required, both in the cardiac cavity, in which there is a circular movement of the contents, and in the pyloric a considerable degree of pressure is employed.

In proof of the delicacy of this process, if too much alcohol gets into the compost it puts a stop to it. Of this no better proof can be given than what happens when a number of young men, while the organs of digestion are in the highest degree efficient, commit a debauch: next morning their stomachs most frequently reject all that had been eaten the

day before, in the same state it had been swallowed, both animal and vegetable, or at least very little acted on by the gastric juice.

Whether the condiments taken with the food by man, and instinctively by many animals, take any part in rendering the compost more readily digested, or only stimulate the nerves of the stomach, and in that way give activity to the actions of the arteries, is perhaps not known; but the last is most probable, since they are not absolutely required; and in what is so essential to the well-being of the animal, nothing would be left to chance. It is, however, certain animals fatten under their use. Aldermen grow fat on well-seasoned turtle. Sheep and cattle improve in a salt marsh. Pigeons, when salt is given. Birds in hot climates, upon Cayenne-pepper. But there is no proof that the condiments form any part of the compost itself, so as to further or accelerate the process of digestion. The use of condiments and a variety of food induce an appetite to receive more into the stomach than could be done from one single dish.

With a view to ascertain what weight of food is usually received into the stomach, by those who indulge in the gratifications of the table, I put it to the test of experiment: and on one occasion, —

Three gentlemen, who were weighed when they sat down, and again when they got up, without having made water, increased in weight as follows:

1st Gentleman, 3lb. 1 oz.
2d Gentleman, 2lb. 10oz.
3d Gentleman, 5lb. 4oz.

Two Ladies of the same party:

1st Lady, 1lb. 12oz.
2d Lady, 3lb. 1oz.

The variation in different individuals is very great.

I found upon drinking a pint of liquid, and being weighed, and again after making water, my weight was lessened one pound.

Six gentlemen sat down to a dinner of turtle and venison, after being weighed; they ate and drank with unusual freedom and gaiety for three hours: when re-weighed, the increase was at the greatest seven pounds, in some five. With a view to ascertain the effect of not allowing the contents to remain many hours in the lower intestines, so as to preclude its being converted into fat, at the same time living full and dining out every day: when my weight was fifteen stone twelve pounds, I for thirty-eight days had a pint of water as a glyster thrown up, which emptied the colon every night, and at the end of that time I had lost a quarter of a pound daily, weighing only fifteen stone two pounds. I then left off the glyster and lived as usual; in nine days, I gained three pounds.

Worms in the stomach produce uneasiness, but not material mischief. A gentleman felt uncomfortable for some time in his stomach: a large round worm crept up into the fauces, which he pulled out with his finger, and the uneasiness was removed.

The following fact is so extraordinary, that it is necessary to mention the names to give it credence. General Grose, who was in the Dutch service under the Duke of Cumberland

in the Flanders war, known to General Gage and Lord Amherst, for thirty years never had a passage through his bowels. General Gage observed that he ate heartily, but in two hours left the table and threw up his dinner: he was a healthy man, and able to use exertion like other men. It is much to be regretted that more particulars of this extraordinary case are not recorded. That he was not a fat man is to be implied, since, had he been so, the circumstance could never have been overlooked. Bile must have been mixed with the food when it was brought up; and his stomach, duodenum, the gall-bladder and ducts must have readily allowed of their peristaltic motion being reversed.

What part bile takes in the process of digestion is not known, if any; in ordinary food, probably none whatever: but in the walrus, from the peculiar kind of food and structure of the gall-bladder, something more may be required, but that must take place in the duodenum.

SECT. III.

On Chemical Combinations that take place in the Stomach, producing Disease in that Organ, and becoming injurious to the general Health.

Very early in my professional life, I became aware how little the knowledge and acquirements of one man are equal to the study of anatomy and physiology ; much less, to enquire into the more intricate researches of the animal economy.

When a student in anatomy, I applied in all my difficulties in anatomy to Mr. Cummings, the watchmaker, who had more knowledge in the principles of mechanics than any other man in London. When unable to make out the uses of parts of the human body, I resorted to Mr. Ramsden, who had a more scientific turn of mind than the best educated mathematicians.

Having strongly felt the advantage thus early of consulting living authorities, I was led in my riper years to associate with Mr. Hatchett and Mr. Brande, both highly eminent for their chemical acquirements. From Mr. Hatchett's labours I gained a knowledge of the component parts of bone and shell, with which he was not himself intimately acquainted, till pressed by me into the investigation.

In the year 1812, I formed a society with my friend, Mr. Charles Hatchett, Sir Humphry Davy, Professor Brande,

Mr. Brodie, the conservator of the Hunterian Museum, and Mr. Children, under the name of "Animal Chemistry." Mr. Hatchett was perpetual president; Mr. Brande, secretary. We met four times a year, twice at Mr. Hatchett's in the summer, and at my house twice in the winter. Sir Joseph Banks was so much pleased with the plan, that he became a member, as well as Mr. Cavendish.

It was from the joint labours of the members that I acquired all my knowledge of the animal economy; digestion, and the wrong actions produced in the stomach by the formation of uric acid. Sir Joseph Banks, with a view the better to promote scientific research, begged that the society might be affiliated to the Royal Society, and all our communications that were of sufficient importance to be presented from the body to be read at the Royal Society, a regulation which was never afterwards deviated from.

This Society, after flourishing for thirteen years, was in the year 1825 broken up by the president resigning the chair, which induced the secretary and myself to withdraw ourselves, and the Society became dissolved.

I have thought it right to say thus much respecting a very valuable Society, of which there will never probably be any other record; more particularly as the following sections of this chapter contain nothing more than the joint labours of Mr. Brande, Mr. Children, and myself, which these my colleagues have allowed me to republish in this place.

From these materials I shall first select my observations on a new fact respecting the crystallization of uric acid on filaments of mucus in the living body, which the Animal Chemistry Society thought of sufficient importance to lay

before the Royal Society, but the Council thought not to merit publication. It may not deserve a place in the Philosophical Transactions, simply as a fact in philosophy; but as its being known led immediately to a mode of relieving the very distressing symptoms it produced in the parts on which the crystals were deposited, thus led away from the scientific view of the subject, our judgment became biassed by the circumstance of being enabled to relieve the sufferings incident to humanity from disease; and I confess myself actuated by the same feelings in bringing the communication forward in this place.

There is no subject connected with Animal Chemistry that can be more interesting to the medical practitioner, than an enquiry into the different forms which the crystals of uric acid put on, according to the different circumstances in which the crystallization takes place; since the degree of irritation produced by a calculus composed of such crystals will be small when the external surface is smooth, and proportionally severe according to its irregularity.

When the uric acid is separated from the urine in the pelvis of the kidney, it appears to have very little animal mucus combined with it, and the aggregate of the crystals deposited at the same moment unite together, forming a sphere usually of the size of a pin's head; and when one of these spheres is formed, its shape makes it little likely to be united to the next similar descent, so that any number of these spheres pass from the pelvis into the ureter, and are expelled with the urine. I have seen one hundred such spheres voided at one time, without pain, from the roundness of their form; and I consider those individuals

to have little chance of having a stone in the bladder, unless they lead a very sedentary life, or are arrived at an advanced age.

Misled by the frequency of these small spherical calculi, I considered all red sand to be more or less of this description; but in this I find I was mistaken, as in the following case the crystals of uric acid, although very minute, were the cause of great suffering to the patient, from the irregularity of their surface.

A young lady occasionally voided red sand, and at these times had excruciating pain; but the sufferings she went through, both in voiding the water, and from spasms in the neck of the bladder and occasional suppressions of urine, were not considered to arise from that cause, but internal inflammation, whether in the kidney, or ureter, or neck of the bladder. Some of this sand, which was voided in unusual quantity, was brought to me for examination. Magnified by a single lens, I found it to consist of very small spiculated crystals, formed upon a filiment of mucus, and exactly resembling the crystals of sugarcandy as they appear when allowed to form upon a string.

This appearance, which was new to me, had not been met with by any of my associates, the members of the Animo-Chemical Society, and it appeared to them, as well as myself, an important fact, as it explained the circumstance of red sand, when not combined with filiments of mucus, giving no irritation to the surfaces it passes over in the body, but, when crystallized upon such filiments, producing the same degree of irritation as the spicular crystals of triple phosphates; and also led to a mode of preventing such symp-

toms in future, by giving alkalies in a caustic state, which destroying the mucus put an end to the formation of such filiments.

Mr. Bauer's microscopical drawings, which are annexed, not only explain the facts that have been stated, but also exhibit the different appearances crystals of uric acid put on according to the circumstances in which the crystallization takes place.

SECT. IV.

Professor Brande's Experiments on Calculi.

Having given my own observations on the crystallization of uric acid, I shall follow them by Mr. Brande's, my fellow labourer upon this subject, given in the first instance to the Society for Animal Chemistry; and which I have his permission to republish, to increase the evidence on this interesting subject: it is in the form of a letter to me.

"Dear Sir,

Having availed myself of the opportunity you procured for me, of making a chemical examination of the calculi contained in the Hunterian Museum, as well as those in your own collection, I herewith send you an account of what I have done.

The collection which I have examined is not only uncommonly large, but the greater part of the specimens have histories of the cases annexed to them.

This circumstance enabled me not only to ascertain the situations in which the calculi were found, but likewise many of the circumstances attendant on their formation.

I have, therefore, endeavoured to form an arrangement upon these principles, with a view to render the subject more clear and perspicuous.

First Division.

Of Calculi formed in the Kidneys, and voided without having afterwards undergone any change in the Urinary Passages.

These have the following properties: —

They are of a brownish yellow colour, sometimes of a greyish hue, which seems to arise from a small portion of dry mucus adhering to their surface.

They are entirely soluble in a solution of pure potash; and, during their solution, they seldom emit an odour of ammonia.

When heated to dryness, with nitric acid, the residuum is of a fine and permanent red colour.

When exposed to the action of the blowpipe, they blacken, and emit a strong odour of burning animal matter, very different from that of pure acid. This arises from a variable proportion of animal matter which they contain, and which occasions the loss in the analysis of these calculi. Its relative quantity is liable to much variation, as may be seen from the following statements: —

A calculus from the kidney, weighing seventy grains, was dissolved in a solution of pure potash. A quantity of muriatic acid (rather more than sufficient for the saturation of the potash) was added, and the precipitate of uric acid thus obtained weighed when dried forty-five grains.

No other substance, except animal matter, which was evident on attempting to obtain the muriate of potash.

could be detected; consequently, the composition of this calculus was as follows:—

Uric acid - - -	45 grains.
Animal matter - -	25
	70

This is the largest proportion of animal matter which I have met with.

A small calculus from the kidney, weighing 3·7 grains, afforded by a like treatment 3·5 grains of uric acid; so that it was nearly a pure specimen of that substance.

The largest calculus of this kind which I have examined weighed seventeen grains: much larger have been found, but there is no evidence of their not having remained in the urinary passages for some considerable time. Thus, Dr. Heberden mentions one weighing twenty-eight grains. *

It often happens that the ingredients are not united together, so as to form a calculus; but are voided in the state of a fine powder, commonly termed sand. This consists either of uric acid, or of the ammoniaco-magnesian phosphate, alone, or with the phosphate of lime.

I was induced to believe that the last-mentioned substances, although the production of the kidneys, and held in solution, are never met with in a separate state, until the urine has been at rest; and, therefore, calculi from the kidneys are never composed of the phosphates.

In a few instances, calculi from the kidneys, composed of oxalate of lime, are voided; but this is a very rare occur-

* Comment. on the Hist. and Cure of Diseases, 3d edit. p. 88.

rence. Of three preserved in the Hunterian collection, two are extremely small and hard, having an appearance of being made up of several smaller calculi, of a dark brown colour; the third is of the size of a small pea, its surface smooth, and of a grey colour, and not very hard.

Second Division.

Of Calculi which have been retained in the Kidney.

When one or more of the calculi described in the preceding section are detained in the infundibula or pelvis of the kidney, it frequently happens that they increase in that situation to a considerable size.

This increase is of two kinds.

1. When there is a great disposition to the formation of uric acid, the calculus consists wholly of that substance and animal matter, so as frequently to form a complete cast of the pelvis of the kidney.

2. Where there is less disposition to form uric acid, the external laminæ are composed of the ammoniaco-magnesian phosphate and phosphate of lime.

In one instance, a small uric calculus had been deposited in the kidney, in such a situation that its upper surface was exposed to a continual stream of urine, upon which beautiful crystals of the triple phosphate had been deposited. It would therefore seem that, under common circumstances, a stream of urine, passing over a calculus of uric acid, has a tendency to deposit the phosphate upon it.

Third Division.

Of Calculi of the Urinary Bladder.

Calculi met with in the bladder are of four kinds :—

1. Those formed upon nuclei of uric acid from the kidney.

2. Those formed upon nuclei of oxalate of lime from the kidney.

3. Those formed upon sand or animal mucus deposited in the bladder.

4. Those formed upon extraneous bodies introduced into the bladder.

They were arranged under the following divisions :—

1. Calculi, which, from their external appearance, consist chiefly of uric acid.

These calculi vary in colour from a deep reddish brown to a pale yellowish brown. They are either entirely soluble in a solution of pure potash, or nearly so. During their solution, they frequently emit the odour of ammonia. When nitric acid is added to their alkaline solution, a precipitate possessing the properties of uric acid is obtained.

2. Calculi, composed chiefly of the ammoniaco-magnesian phosphate, or of phosphate of lime, or of mixtures of the two.

These calculi are externally of a whiter appearance than the former. Some perfectly white, others grey, occasionally exhibiting small prismatic crystals upon their surface : others, again, soft and friable, a good deal resembling chalk. They are further characterized by their solubility in dilute muriatic acid.

3. Calculi, containing oxalate of lime; commonly called mulberry calculi.

These are distinguished by the difficulty with which they dissolve in dilute acids, by their hardness, and by leaving pure lime, when exposed to the action of a blowpipe.

In the examination of these calculi, I was struck with the small number of those strictly belonging to the first division, having been led from the account of Fourcroy and Vauquelin*, and the experiments of Dr. Pearson†, to believe that calculi composed of pure uric acid were by no means unfrequent.

The greater number of the calculi examined by the former chemists, are stated to be completely soluble in the fixed alkaline lees: and of 300 examined by Dr. Pearson, a large proportion is said to consist of uric acid.

The following is a statement of the composition of the different calculi found in the bladder, which I have examined.

16 were composed of uric acid.
45 were composed of uric acid, with a small relative proportion of the phosphates.
66 were composed of the phosphates, with a relatively small proportion of uric acid.
12 were composed of the phosphates entirely.
5 were composed of uric acid, with the phosphates and nuclei of oxalate of lime.
6 were composed chiefly of oxalate of lime.

150

* Annales de Chimie, xxxii. 218.

† Phil. Trans. 1798, 37.

To injure those calculi as little as possible, they were carefully cut through with a fine saw, and a portion of the whole cut surface removed by a file: in this way all the different ingredients of the calculi were obtained.

In the experiments upon uric calculi from the bladder, I found, in most instances, a far more considerable loss in attempting to obtain their pure uric acid, than in the kidney calculi, which led me to suppose that they contained urea, and that the presence of this substance, with some of the salts of urine, and with small portions of the ammoniaco-magnesian phosphate, was the cause of the occasional evolution of ammonia, when treated with the fixed alkalies, and of their easy solubility in those substances.

To determine this point, a small calculus, weighing twenty-five grains, and of the species commonly supposed to consist of urate of ammonia*, was digested for two hours with water in a very moderate heat. The water, which had assumed a pale yellow colour, was filtered off, and fresh water added to the residuum three successive times, when it appeared that every thing soluble in that fluid was separated. The insoluble part of the calculus being now carefully dried and weighed, was found to have lost fifty-five grains.

The aqueous solution was evaporated, by a gentle heat, nearly to dryness, and a substance was obtained having all the properties of urea, in combination with a small portion of muriate of ammonia, and of the ammoniaco-magnesian phosphate.

* Fourcroy observes, that urate of ammonia is easily detected by its rapid solubility in the fixed alkalies, and the odour of ammonia, which is perceived during its solution. Vide Thomson's Syst. of Chem. vol. v. p. 691.

Sixty grains of another calculus of a considerable size, supposed, from a superficial analysis, to consist of nearly pure urate of ammonia, were digested, at a low temperature, in one ounce of alcohol. In an hour, the alcohol was decanted off; and fresh portions were added successively, as long as it appeared to act upon the calculus, which, after having been carefully dried in a temperature below 212°, weighed 54·8 grains; so that 5·2 grains had been taken up by the alcohol.

On evaporating the alcoholic solutions, a substance was obtained having all the properties of urea, with a small portion of saline matter, probably muriate of ammonia, as, by the addition of potash, a slight ammoniacal odour was perceptible: its quantity, however, was too minute for accurate examination.

The remaining portion of the calculus, weighing 54·8 grains, was treated with small portions of acetic acid, by which six grains of the ammoniaco-magnesian phosphate were obtained.

The part of the calculus remaining after this treatment, weighing 48·8 grains, was perfectly soluble in a solution of pure potash: it emitted no ammoniacal odour, when acted upon by the alkali, and possessed the properties of pure uric acid.

The following, therefore, is the composition of this calculus:—

Urea, and muriate of ammonia	-	5·2 grains.
Ammoniaco-magnesian phosphate	-	6
Uric acid - - -	-	48·8
		60

From these, and many similar experiments upon other calculi, hitherto generally supposed to consist of urate of ammonia, I am induced to believe that the evolution of ammonia depends in all instances upon the decomposition of the ammoniacal salts contained in the calculus, more especially of the ammoniaco-magnesian phosphate, and that no substance which can be called *urate* of *ammonia* exists in calculi.

The mulberry calculus (oxalate of lime) I have but rarely met with. In those preserved in the Hunterian Collection, there is a large relative proportion of phosphate of lime, and of uric acid. The purest of them afforded

Oxalate of lime - - -	65 grains.
Uric acid - - - -	16
Phosphate of lime - - -	15
Loss in animal matter - -	4
	100

When calculi of the urinary bladder increase to a very large size, they are generally composed of two, or even three of the above-mentioned varieties; the ammoniaco-magnesian phosphate being situated externally and in the greatest abundance.

The largest calculus which I have seen weighed, when recently removed from the bladder, twenty-three ounces and twenty-six grains. It consisted of a large mulberry or oxalate of lime calculus, the nucleus of which was uric acid, surrounded by a considerable quantity of the ammoniaco-magnesian phosphate in a very pure state.

Another very large calculus, weighing fifteen ounces and a half, consisted of a nucleus of uric acid, enveloped in the ammoniaco-magnesian phosphate, not, however, pure, but intersected by several laminæ of uric acid.

Four distinct substances are extremely rare in calculi. I have seen one in which the uric acid, the ammoniaco-magnesian phosphate of lime, and the oxalate of lime, were all in perfectly separate and distinct layers.

Four calculi, having the following extraneous substances for their nuclei, were examined : —

1. A common garden pea.
2. A needle.
3. A hazel nut.
4. A part of a common bougie.

In the two first instances, the calculous depositions were of a pale grey colour, inclining to white ; soft and friable in their texture, and entirely soluble in muriatic acid.

The composition of the first was as follows : —

Phosphate of lime - -	65 grains.
Ammoniaco-magnesian phosphate	28
Loss - - -	7
	100

Of the second : —

Phosphate of lime - -	45 grains.
Ammoniaco-magnesian phosphate	38
Oxalate of lime - -	12
Loss - - -	5
	100*

* It appears that in this case there had been an accidental disposition to the formation of oxalate of lime.

The deposition of calculous matter upon the bougie was covered with blood, and in a very small quantity, the bougie having been removed by an operation soon after it had passed into the bladder. It appeared to consist chiefly of phosphate of lime.

The incrustation upon the hazel nut was also destitute of uric acid.

Fourth Division.

Of Calculi of the Urethra.

All those that were examined had escaped from the bladder while very small, and had afterwards lodged in the membranous part of the urethra, where they increased in size, and formed a cavity in which they were more or less embedded.

Two of these calculi were broken.

The fragments consisted, in one instance, of ammoniaco-magnesian phosphate, and phosphate of lime, with a small portion of uric acid; and in the other, the fragments were composed entirely of the ammoniaco-magnesian phosphate.

The third calculus was of a very remarkable appearance; its form being that of a perfect sphere, about half an inch in diameter. It was coated with small but very regular crystals of the triple phosphate in its purest state. On account of the singularity of the form and external appearance of this calculus, it was not sawn through; the nucleus, in all probability, is a small kidney calculus, which, lodging in the urethra, has become coated with triple phosphate.

Fifth Division.

Analysis of Calculi from other Animals.

I. The Horse.

1. From the kidney.

A very large calculus from the kidney of a horse was composed of

Phosphate of lime - -	76
Carbonate of lime - -	22
	98

2. From the bladder.

This calculus, also, was of a very large size; its weight, when perfectly dry, nine ounces and a half; its external surface very irregular, of a reddish brown colour, and covered with minute crystals of the ammoniaco-magnesian phosphate. On making a section of it, the internal structure exhibited a radiated appearance, and was of a light brown colour. It consisted of

Phosphate of lime - - - -	45
Ammoniaco-magnesian phosphate -	28
Animal matter - - - -	15
Carbonate of lime - - - -	10
	98

In another case the bladder of a horse was found to be nearly full of sand, the composition of which was as follows:

Phosphate of lime - - -	60
Carbonate of lime - - -	40
	100

II. The Ox.

A number of small calculi, from the size of a pea downwards, are not unfrequently found in the bladder of the ox. Those in the Hunterian Collection are of a pale brown colour, and of the size just mentioned: some of them have the mulberry appearance.

They consist of the carbonate of lime and animal matter; which last substance retains the form of the calculus after it has been acted upon by diluted acids.

III. The Sheep.

A calculus from the kidney of a sheep was composed of

Phosphate of lime - - -	72
Carbonate of lime - - -	20
Animal matter - - -	8
	100

IV. The Rhinoceros.

The urine of this animal is exceedingly turbid at the time it is voided, and, when allowed to remain at rest, deposits a very large proportion of sediment, which consists of carbonate of lime, with small portions of phosphate of lime, and animal matter.

V. The Dog.

A large calculus from the bladder of a dog twenty years old, weighing sixteen ounces, was extremely hard, and of a grey colour; when cut through, it exhibited a nucleus about the size of a hazel nut, partly made up of concentric layers of phosphate of lime, and partly of crystals of the ammoniaco-magnesian phosphate. The part of the stone surrounding the nucleus consisted of

Phosphate of lime - - -	64
Ammoniaco-magnesian phosphate -	30
Animal matter - - - -	6
	100

Sand taken from a dog's bladder was of a grey colour, and contained

Carbonate of lime - - -	20
Phosphate of lime - - -	80
	100

VI. The Hog.

A calculus from the bladder of this animal weighed nineteen drachms. It was of a pale grey colour, inclining to white, and so hard that it was with difficulty cut through. Its internal structure was uniform, and there was no appearance of a nucleus. It was composed of

Carbonate of lime - - -	90
Animal matter - - - -	10
	100

VII. The Rabbit.

A calculus from the rabbit's bladder, weighing four drachms, was of a dark grey colour, and appeared as if composed of several smaller calculi.

It consisted of

Phosphate of lime - - -	39
Carbonate of lime - - -	42
Animal matter - - - -	19
	100

Sixth Division.

General Inferences.

It appears from the preceding observations, that calculi formed in kidneys, and immediately voided, are almost always composed of uric acid; and that the phosphates are very frequent ingredients in calculi of the bladder, more especially in those which, from their situation, have been exposed to a continual current of urine: they also uniformly are deposited upon extraneous substances introduced into the bladder, but appear never to form nuclei.

In what is commonly called a fit of the gravel, a small uric calculus is formed in the kidney, and passes along the ureter into the bladder.

It is found from observation that, for some time after a stone has passed from the kidney, the urine is generally unusually loaded with uric acid, and deposits that substance

upon the nucleus now in the bladder. When this period, which is longer or shorter in different individuals, has elapsed, the subsequent addition to the calculus consists principally of the phosphates.

Where the disposition, therefore, to form uric acid in the kidneys is very great, and permanent, the calculus found in the bladder is principally composed of uric acid; but where this disposition is weak, and of short duration, the nucleus only is uric acid, and the bulk of the stone is composed of the phosphates.

Where the increased secretion of uric acid returns at intervals, the calculus is composed of alternate layers of uric acid and the phosphates.

Other small calculi, being formed in the kidney, make their way into the bladder, and afford fresh nuclei; so that several calculi are sometimes found in the same bladder, and their composition is, usually, nearly the same.

In other cases it happens that a constant increased secretion of uric acid is going on from the kidneys, only in small quantity, which will be more uniformly mixed with the phosphates deposited in the bladder; and, when the uric acid predominates, the species of calculus denominated improperly *urate* of *ammonia* will be produced.

We are entirely ignorant of the cause of the formation of the oxalate of lime, or mulberry calculus. I have frequently looked for oxalate of lime in the urine of calculous patients, but have never been able to detect it; and as it does not exist in healthy urine, it must be regarded as a morbid secretion. Its mode of formation seems to resemble that of uric acid, since small kidney calculi, composed of oxalate of lime, have in a few instances been voided; and in these

cases, as far as my enquiries go, the persons have been much less liable to a return of the complaint, than where uric calculi have been voided.

In some rare instances we meet with calculi of the bladder which are destitute of uric acid and of oxalate of lime, the nucleus being composed of a little loosely agglutinated ammoniaco-magnesian phosphate, and the whole calculus consisting of that substance, with variable portions of phosphate of lime. In two cases I have met with calculi of this kind, composed of the triple phosphate only: they seem to be entirely formed in the bladder.

Having taken this short view of the formation of calculi, I shall now enquire into the action of solvents, employed either with a view of effecting their solution, or of preventing their formation and increase.

Solvents are of two kinds,—

1. Alkaline; 2. Acid.

In the exhibition of these, the practitioner is usually guided by the chemical composition of the calculous matter voided by urine.

The different kinds of gravel, voided by persons labouring under calculous complaints, may be classed in two divisions:

1. *Uric acid*, either in a pure state, or with a very small proportion of the phosphates;

2. The *phosphates*, either pure, or with a small proportion of uric acid.

The first species, which generally appears in the form of minute crystalline grains, of a reddish brown colour, or of an impalpable brown powder, is either entirely soluble in pure alkaline solutions, not emitting an ammoniacal odour,

in which case it consists of pure uric acid; or it does emit an ammoniacal odour, and is not entirely soluble, in which case it contains the triple phosphate of ammonia and magnesia.

When this substance is observed in the urine, the alkalies are recommended. They are exhibited either in a pure state, or as carbonates; and in such instance the uric sediment generally diminishes rapidly; and, during the continued use of alkaline medicines, occasionally disappears altogether.

It, however, frequently happens that the matter voided is not diminished in quantity by the use of alkalies, but that its form and composition are altered, and that it assumes the appearance of a grey powder, and is composed of uric acid with variable portions of the ammoniaco-magnesian phosphate.

From these facts, therefore, it cannot be doubted that the internal exhibition of alkalies often prevents the formation of uric acid, and hence most likely prevents the increase of a calculus in the bladder, as far at least as uric acid is concerned; but it has also been supposed that the alkalies are capable of acting upon the stone itself, and even of effecting its complete solution. It is true that if we immerse a calculus, composed of uric acid, in a dilute solution of caustic alkali, it will be slowly acted upon, and after some time entirely dissolved.

If, however, we attend to what would take place in the body, we shall find the circumstances very different.

That alkaline carbonates and sub-carbonates exert no action upon uric acid, I consider to be completely esta-

blished, both by the experiments of several eminent chemists, and those I have myself made upon the subject; and as there is at all times a quantity of uncombined acid in the urine, it follows that, although the alkali may arrive at the kidneys in its pure state, it will there unite with the uncombined acid, and be rendered incapable of exerting any action upon the calculus in the bladder. Besides phosphoric acid, the urine always contains a quantity of uncombined carbonic acid. This is proved by placing a quantity of recently voided urine under the receiver of an air-pump: during the exhaustion, a large quantity of carbonic acid gas makes its escape; and, when urine is distilled at very low temperatures, carbonic acid gas is given off; and, also, when lime-water is poured into urine, a precipitate appears, consisting of phosphate and carbonate of lime.

Lime-water, on account of the insoluble compounds which lime forms with carbonic and phosphoric acids, is even more objectionable as a solvent than the alkalies.

It may, however, be said, that, if these means prevent the increase of a calculus, material relief is afforded to the patient. How far the exhibition of alkaline remedies can be recommended upon these grounds will appear, when the circumstances which attend the formation of the second species of calculous sediment or deposition in the urine are considered.

The ammoniaco-magnesian phosphate appears under two forms: it is either voided in a solid state, or in solution. In the former case it bears a good deal of resemblance to a white sand, and is frequently mixed with variable proportions of phosphate of lime: in the latter, it makes its

appearance after the urine has remained undisturbed for some hours in an open vessel, generally in the form of a fine pellicle, or of crystalline laminæ, which, when collected and dried, bear some resemblance to boracic acid.

Its putting on this form is accounted for, from its being held in solution, in the first instance, by carbonic acid; and, as this flies off, the triple salt makes its appearance. If a portion of the urine be preserved in a phial closely stopped, the carbonic acid cannot escape, and consequently no phosphate is observed to separate. There is also a quantity of phosphoric acid present, which keeps another portion of the ammoniaco-magnesian phosphate, and also some lime (in the state of super-phosphate of lime), in solution.

It is therefore obvious that, whenever the urine is deprived of a portion of the acid which is natural to it, the deposition of the triple phosphate, and the phosphate of lime, more readily takes place: this is effected by the exhibition of the alkalies.

It may therefore be asserted that, although alkaline medicines often tend to diminish the quantity of uric acid, and thus to prevent the addition of that substance, in its pure state, to a calculus in the bladder, they favour the deposition of the phosphates.

It cannot be doubted that the alkalies reach the bladder. In some cases, where large doses of sub-carbonate of potash have been exhibited, I have seen evident traces of it in the urine.

When the phosphates only are voided, it has been proposed to dissolve the calculus by the exhibition of acids, and more especially the muriatic acid.

During the use of the muriatic acid, the phosphates are either diminished or disappear altogether; and even sometimes the urine acquires an additional acidity. A solution of that part of the calculus which consists of the phosphates might therefore be expected: but even then the nucleus of uric acid would remain; and thus a great deal of time would be lost, without any permanent advantage.

I have also occasionally remarked that, during the use of acids, the uric acid re-appears, and even seems to be augmented in quantity.

Attempts have been made, at different times, to effect the solution of calculi by the injection of solvents into the bladder. This subject has been lately revived by Fourcroy and Vauquelin, who, in their paper on the composition of calculi, lay down rules for its practice. Independent, however, of the impossibility of ascertaining the composition of calculi with sufficient accuracy, it is obvious that, were the composition of the surface of the calculus known, the frequent introduction of an instrument into the bladder, and the long continuance of the process, which would be necessary even where the calculi are small, are insurmountable objections; and, whenever this mode of treatment has been adopted, it has speedily been relinquished, as it always aggravates the sufferings of the patient.

It has been shown that, in the majority of cases, the nuclei of calculi originate in the kidneys, and that of these nuclei by far the greater number consists of uric acid; the good effects, therefore, so frequently observed during the

use of an alkali, arise, not from any actual solution of calculous matter, but from the power which it possesses of diminishing the secretion of uric acid, and thus preventing the enlargement of the calculus; so that, while of a very small form, it may be voided by the urethra."

SECT. V.

Observations confirming Mr. Brande's Experiments on Calculi.

That calculi in the human bladder are not dissolved by the internal use of acid or alkaline medicines, is an opinion I have long entertained; but the grounds of failure I was not acquainted with till I received Professor Brande's explanation, contained in the foregoing letter: I only knew by experience that, to whatever extent the medicine was given, no such effect was produced. The circumstance of portions of the exterior laminæ of calculi which had been extracted from the bladder by the operation for the stone, after the person had long been under a course of alkaline medicines, being softer than the parts nearer the centre, has always been considered as a proof of the action of the medicines upon the calculi, and led to the belief that when the calculus is small it may be occasionally dissolved. This Professor Brande has now proved to be a deception, by ascertaining that this softer portion is no part of the original calculus, but a newly formed substance, in which the uric acid is not deposited in crystals, but mechanically mixed with the phosphates, and the animal mucus in the urine.

Having met with cases, which confirm Professor Brande's observations, it will be satisfactory to state them, as they

may assist in doing away many erroneous notions generally entertained on this subject.

The opinion that calculi in the human bladder have been entirely dissolved, has received its principal support from instances having occurred, and those by no means few in number, where the symptoms went entirely away while the patients were using alkaline medicines, and never afterwards returned. This evidence appears to be very strong, but it will be found, from the following cases, that it is not so in reality; since the fallacy has been detected in all the instances in which an opportunity was afforded of examining the bladder after death. Two of these I shall particularly notice, because they were published during the patients' life-time in proof of the stone having been dissolved.

Both patients were great sufferers from the symptoms of stone for many years; but when they arrived at the age of sixty-eight, or there about, the symptoms entirely left them. The one had been taking the saline draught in a state of effervescence, under the direction of the late Dr. Hulme: the cure was attributed to this medicine, and the case was published in proof of its efficacy. When the patient died I examined the bladder, and found twenty calculi; the largest of the size of a hazel nut, the others smaller.

It appeared that the going off of the symptoms, had arisen from the posterior lobe of the prostate gland having become enlarged (a change which it frequently undergoes about that period of life); and having formed a barrier between the calculi and the orifice of the bladder, so that they no longer irritated that part, either in the act of making water, or in the different movements of the body.

but lay in the lower posterior part of the bladder without producing any disturbance. Their number prevented the pressure from being great upon any one part of the intestine immediately behind the bladder, and their motion on one another rendered their external surface smooth, and probably prevented their rapid increase. The other patient was under a course of Perry's lixivium; and when the symptoms went away, he published the case in proof of the efficacy of that medicine in dissolving the stone. I examined the bladder after death, and found fourteen calculi; the largest of the size of a nutmeg, the others smaller.

There was the same enlargement of the posterior lobe of the prostate gland, and the calculi were exactly under the same circumstances as in the former case.

In several cases in which I have examined the body after death, calculi have been found enclosed in cysts, formed between the fasciculi of the muscular coat of the bladder, so as to be entirely secluded from the general cavity, and therefore had not produced any of the common symptoms of stone. I have seen in the same bladder, two, three, and even four such cysts, each containing a calculus of the size of a walnut.

It is a circumstance deserving notice, that in the case which gave celebrity to Mrs. Stevens's medicine, and procured her a remuneration from parliament, the bladder was not examined after death.

That calculi in the bladder do sometimes increase, while the patient is using alkaline medicines, is fully proved by the following examples, which also show that the uric acid

and phosphates are formed in different proportions, according to the peculiarities of the constitution : —

A gentleman who suffered from symptoms of stone was sounded, and a stone was found in his bladder. I put him on a course of alkaline medicines, and he voided a small compact calculus, composed of uric acid, and evidently formed in the kidney. He was desired to persist in the use of the medicine, which he did at intervals, for four or five years, suffering occasionally in a slight degree, but he did not pass any more calculi. He died at the age of seventy-five. On examining the bladder, its whole cavity (the capacity of which was equal to a pint measure) was completely filled with soft, light, spongy calculi, 350 in number, and of different sizes, from that of a walnut to a small pea. They were composed of a mixture of uric acid in powder, the phosphates, and animal mucus ; and differed so much from the calculus voided soon after the patient began the use of alkalies, that they appear to have been formed after that period, in the manner mentioned by Professor Brande.

A gentleman, who was found to have a stone in the bladder, was persuaded that it was so small that it might be dissolved, and, with this view, he took the fossil alkali, both in its caustic and solid state, for about three months ; but at the end of that period the symptoms were increased, and he submitted to have it extracted by an operation. On examining the calculus, after I had extracted it, I found the external part, for the thickness of one tenth of an inch, entirely composed of triple phosphate, in a state of perfect spiculated crystals, so as to present a very rough irritating surface to the internal membrane of the bladder, while the

inner parts of the calculus were made up of a mixture of uric acid and phosphates, so that the alkali had prevented the formation of uric acid, but the phosphates were deposited more rapidly than before.

A gentleman, in whose urine the uric acid appears in a solid form, immediately after it is voided, has the same appearance in the urine, even when nine drachms of soda dissolved in water impregnated with carbonic acid are taken in twenty-four hours; so that, in this instance, the alkali does not even counteract the formation of uric acid.

Mr. Brande's experiments, and my comments, have a place in the Philosophical Transactions, but have never before been given to the public at large; which is now done, not only on account of their value, but to assert the author's right to them, since many have since been published without due acknowledgment.

SECT. VI.

Observations on the Effects of Magnesia in Calculous Complaints, made by Mr. Brande at my request, and originally published in the Philosophical Transactions.

"Sir Everard Home's enquiries into the functions of the stomach, and his discovery of liquids passing from the cardiac portion into the circulation of the blood *, led him to consider, that the generality of calculous complaints might possibly be prevented, by introducing into the stomach such substances as are capable of preventing the formation of uric acid, and that this mode of treatment would have many advantages over the usual method, which consists in attempting to dissolve the uric acid after it is formed.

He consulted Mr. Hatchett on the substance most likely to produce this effect, and asked if magnesia, from its insolubility in water, was not well adapted for the purpose, as it would remain in the stomach, until it should combine with any acid, or be carried along with the food towards the pylorus.

Mr. Hatchett knew of nothing more likely to produce the desired effect; and on putting this theory to the test of experiment, it was found, by a very careful examination of

* Philos. Trans. 1808.

the urine, that in several instances where there was an increased formation of uric acid, magnesia diminished it in a much greater degree than had been effected by the use, and that a very liberal one, of the alkalies in the same patient.

This circumstance led Sir Everard Home to wish for a more complete investigation of the subject, and he requested me to assist him in the prosecution of it. Since that time many opportunities have occurred of carrying on the enquiry, during an attendance on patients labouring under calculous complaints.

It is proposed to lay the results of our joint labours before the public, with a view to establish a fact of so much importance in the treatment of those diseases.

The four following cases include the principal varieties of the disorder which have been met with; and are therefore selected from among many others, to prevent unnecessary repetitions. In each of them the urine was exceedingly carefully analysed.

CASE I.

A GENTLEMAN, sixty years of age, who had been in the habit of indulging in the free use of acid liquors, had repeatedly passed small calculi, composed entirely of uric acid. His urine, immediately after being voided, deposited at all times a considerable quantity of that substance, in the form of a red powder, and occasionally in larger crystals.

Nine drachms of subcarbonate of soda, dissolved in water highly impregnated with carbonic acid, and taken in the course of the day in three doses, appeared to have no effect whatever on the formation of uric acid. The red sand was deposited as usual, and the small calculi continued to form.

On account of the inefficacy of this medicine, he was advised to try the vegetable alkali; and three drachms of subcarbonate of potash, dissolved in water slightly impregnated with carbonic acid, were taken at similar intervals.

The deposit of uric acid in the urine was now somewhat diminished; but during this free use of alkalies, which, with little interruption, was persevered in for more than a year, the small calculi still continued to be voided.

The very unusual disposition to form uric acid, and the complete failure of the common alkaline medicines, rendered this case particularly favourable for the trial of magnesia, as it would afford an opportunity of comparing its effects with those of the alkalies.

Previous to giving the magnesia, the urine was examined, to ascertain the quantity of uric acid it contained; this being done, the patient was directed to take fifteen grains of magnesia three times a day, in an ounce and a half of infusion of gentian. In a week the uric acid was found, by examining the urine, to have diminished in quantity, and after the first three weeks, it was only occasionally met with.

The use of magnesia has been persevered in for eight months, during which time no calculi have been voided, nor has there been any material deposit in the urine.

The patient was extremely subject to heartburn, and he likewise complained of a sense of weight and uneasiness about the region of the stomach, both of which symptoms have disappeared.

CASE II.

A GENTLEMAN, about forty years of age, had, during four years, occasionally voided considerable quantities of uric acid, in the form of red sand, and had once passed a small calculus.

His urine was generally more or less turbid; and after taking any thing which disagreed with his stomach, even in a slight degree, the red sand often made its appearance. He had never used the alkalies, nor any other medicine, to alleviate his disorder; he was consequently desired to take a drachm and a half of subcarbonate of soda, dissolved in a pint and a half of water highly impregnated with carbonic acid, in the course of the day, and to persevere in this treatment for some time.

On the 30th of January, 1809, he left London, and returned the 6th of March following.

During his absence he had voided rather less uric acid, but had had one severe attack, in consequence of which twenty drops of the solution of pure potash were added to each dose of the soda water. This, however, had not the desired effect; for, on the 10th of March, having taken more wine than usual on the preceding day, he was attacked with pain

in the right kidney; and voided with his urine a considerable quantity of uric acid, in the form of minute red crystals. During the succeeding day he made but little water, which deposited a copious sediment of red sand.

For the removal of this symptom, he was directed to take magnesia, in the dose of twenty grains, every night and morning in a little water. For three successive days his bowels were occasionally relaxed, but afterwards became regular. He persevered in its use for six weeks, without intermission: his urine was several times examined during that period, and contained no superabundant uric acid; and he has not had the slightest return of his complaint, although he has put himself under no unusual restraint in his mode of living.

CASE III.

About the middle of October, 1808, a gentleman, forty-three years of age, after taking violent horse exercise, was seized with pain in the right kidney and ureter. In the course of the night he passed a small uric calculus. For some months previous to this attack he had felt occasional pain in the kidney, but had never voided either calculi or sand. His urine was now always turbid, and occasionally deposited red sand.

On the 28th of October he began the use of soda water, and, for a time, his urine was much improved in appearance: but the uric acid gradually returned; and at the end of

December, notwithstanding the continued use of the soda water, he voided more sand, and his urine was more loaded with mucus, than it had ever been before.

In consequence of these symptoms, on the 3d of January, 1809, he was directed to take twenty grains of magnesia every night.

The urine was examined after the third dose, and the deposit of red sand was diminished in quantity, but it did not disappear entirely, after the magnesia had been taken for three weeks.

About this time (on the 26th of January) he caught cold, and his urine was again very turbid; but this was found to be wholly the effect of mucus, and the symptom soon left him.

On the 30th of January he took 20 grains of magnesia, and repeated it every night and morning, until the 1st of March, when his urine was perfectly healthy, and he left it off.

On the 1st of June he again voided a little uric acid, in the form of red crystalline sand: this attack was attended with a slight pain along the right ureter. He returned to the use of the magnesia, which he took twice a day for three weeks, in the same dose as before, and from that time to the middle of November, there had been no symptoms of a return of the complaint.

CASE IV.

A GENTLEMAN, aged fifty-six, after recovering from a severe fit of the gout, voided constantly a large quantity of mucus in his urine, a symptom which he had never before noticed. There was also, occasionally, abundance of red sand, consisting principally of uric acid; but he had never voided a calculus.

His stomach was uncommonly weak: he was often affected with heartburn, and an almost constant pain in the neighbourhood of the right kidney. He had been in the habit of taking tincture of bark, and other spirituous medicines, from a belief that the pain in the right side arose from gout in the stomach.

He had already attempted to use the alkalies, which had produced such unpleasant sensations in the stomach, that he could not be prevailed upon to try them again in any form.

Under these circumstances, he readily acceded to a new plan of treatment. He was directed to omit the use of spirituous medicines, and take twenty grains of magnesia three times a day in water; but this operating too powerfully upon the bowels, the same quantity of magnesia was taken twice a day only, with an addition of five drops of laudanum to each dose.

This plan was pursued, without intermission, for three weeks; and he received considerable benefit, as far as concerned the state of the stomach, and pain in the region

of the kidney. The urine, which was examined once a week, was also, on the whole, improved; but it occasionally deposited a very copious sediment, consisting of uric acid, with a variable proportion of mucous secretion.

After a further continuance of the use of magnesia for three weeks, the urine was often much loaded with uric acid and mucus; but these appearances, which before the use of the magnesia were constant, are now only occasional, so that the disposition to form a redundant quantity of uric acid is much diminished. It is also deserving of remark, that there has not been the slightest symptom of gout from the time of the last attack, which is more than a year back, a longer interval of ease than this patient has experienced for the last six years.

He has now omitted the regular use of the magnesia; but, on perceiving any unpleasant sensation in the stomach, he returns to it for a week or ten days, and then again leaves it off.

From the preceding cases it appears that the effects of magnesia taken into the stomach are, in many respects, different from those produced by the alkalies, in those patients in whom there is a disposition to form a superabundant quantity of uric acid.

With a view to ascertain their comparative effects on healthy urine, when taken under the same circumstances, the following experiments were made:—

Experiment I.

On Soda.

Two drachms of subcarbonate of soda were taken on an empty stomach, at nine o'clock in the morning, dissolved in three ounces of water, and immediately afterwards a large cup of warm tea.

In six minutes, one ounce of urine was voided; in twenty minutes, six ounces more; and, after two hours, a similar quantity.

The first portion became very turbid within ten minutes after it had been voided, and deposited a copious sediment of the phosphates, in consequence of the action of the alkali upon the urine. It slightly restored the blue colour to litmus paper reddened with vinegar: the alkali, therefore, was not merely in sufficient quantity to saturate the uncombined acid in the urine, and, consequently, to throw down the phosphates, but it was in excess, and the urine was voided alkaline.

The urine voided after twenty minutes also deposited a cloud of the phosphates; but the transparency of that voided two hours after the alkali had been taken was not disturbed.

Here, therefore, the effect of the alkali upon the urine was at its maximum, probably, in less than a quarter of an hour after it had been taken into the stomach, and in less than two hours the whole of the alkali had passed off.

Experiment II.

On Soda with Excess of Carbonic Acid.

The same quantity of soda, dissolved in eight ounces of water very highly impregnated with carbonic acid, was taken under the same circumstances as in the former experiment, and the urine was voided at nearly similar intervals.

The separation of the phosphates was less and less rapid. In two hours after the urine had been voided, there was a small deposit, composed principally of phosphate of lime; there was also a distinct pellicle on the surface, consisting of the triple phosphate of ammonia and magnesia. This appearance, produced by the escape of the carbonic acid, which had before retained the ammoniaco-magnesian phosphate in solution, and which now occasioned its deposition on the surface, is by no means uncommon, even in the urine of healthy persons. In the present instance it appears to prove that carbonic acid passes off from the stomach by the kidneys; for, after taking the alkalies in water, highly impregnated with it, the pellicle is uniformly produced, and is also much more abundant and distinct than under any other circumstances.

In similar experiments with potash, the results were, in all cases, as similar as could be expected in researches of this nature.

Experiment III.

On Magnesia.

Magnesia was taken under circumstances similar to those of the soda in the former experiment. In the quantity of half a drachm, it produced no sensible effect upon the urine during the whole day. When taken in the dose of a drachm at nine o'clock in the morning, the urine voided at twelve o'clock became slightly turbid : at three o'clock the effect of the magnesia was at its maximum, and a distinct separation of the phosphates took place, partly in the form of a film, which, when examined, was found to be triple phosphate of ammonia and magnesia, and partly in the state of a white powder consisting almost entirely of the triple phosphate of lime.

The effect of large doses of magnesia in producing a white sediment in the urine is very commonly known, and has been commonly attributed to the magnesia passing off by the kidneys.

These experiments show that magnesia, even in very large doses, neither produces so rapid an effect upon the urine, nor so copious a separation of the phosphates, as the alkalies: on this its value as a remedy in calculous disorders seems materially to depend.

Experiment IV.

On Lime.

Two ounces of lime-water, taken in the morning upon an empty stomach, with a cup of milk and water, produced no visible effect whatever.

A pint of lime-water, taken at four intervals of an hour each, produced a slight deposition of the phosphates at the end of the fifth hour. The urine voided at the third hour was not at all affected; at the fifth hour, the effect appeared at its height, but was not nearly so distinct as from small doses of soda, notwithstanding the insoluble compounds which lime might be expected to form with the acids in the urine.

The unpleasant taste of lime water, the quantity in which it requires to be taken, on account of the small proportion of the earth which is held in solution, and the uncertainty of its effect, are circumstances which render it of little use, excepting in some very rare cases, where it has been found to agree particularly with the stomach.

The effect of carbonate of lime upon the urine was much less distinct than that of lime-water. At times it produced an effect; but, when taken in very large doses, a slight deposition of the phosphates was produced.

These experiments were repeated upon three different individuals, and there was always a uniformity in the results.

When the medicines were taken some hours after food being received into the stomach, their effects upon the urine were retarded, but not prevented.

The effects of many other substances upon the urine were examined into during this investigation; but they varied so much according to circumstances, that no satisfactory results were produced.

As it is found, in the foregoing experiments, that the effects of soda on the urine are modified by the presence of carbonic acid, the following experiment was made to ascertain whether any sensible effects are produced by that acid on healthy urine.

Twelve ounces of water, highly impregnated with carbonic acid, were taken on an empty stomach, at nine o'clock in the morning. At ten o'clock, about eight ounces of urine were voided, which had a natural appearance; but, when compared with urine voided under common circumstances, was found to contain a superabundant quantity of carbonic acid: this gas was copiously given off, when the urine was gently heated, or when it was exposed under the exhausted receiver of an air-pump.

To a patient who had a calculus of large dimensions extracted from the bladder, composed entirely of the phosphates, and whose stomach did not admit of stronger acids, carbonic acid was given in water. It was found peculiarly grateful to the stomach; and, upon examining the urine during its use, the phosphates were only voided in solution; but, when at any time it was left off, they were voided in the form of white sand.

Additional Observations on the Effects of the Use of Magnesia, made in the Year 1811.

Since these observations were published in the Philosophical Transactions *, many opportunities have occurred, both to Sir Everard Home and myself, of confirming the efficacy of magnesia upon a more extended scale, and of ascertaining the efficient treatment of those cases in which magnesia is effectual, and in which it has even been found to aggravate the complaint.

To bring forward additional evidence in favour of the use of magnesia, and to distinguish the cases in which its use is indicated, from those where it is improper or hurtful, are the principal objects of the present communication, which will be considered in the two following Divisions.

First Division.

The following is the case of a gentleman who suffered from a calculous complaint; during which he was accidentally induced to employ magnesia, the effects of which he has thus described: —

* For 1803, p. 108.

CASE I.

'About twenty-seven years ago I felt a pain in one of my kidneys, particularly when in bed, which continued to increase during six months. I had likewise an occasional sympathetic pain in the testicles, and violent and excruciating pains in the left kidney now became frequent. These attacks were sometimes brought on by stooping to take up something; but, at other times, without any apparent cause. They lasted from twelve to twenty-four hours, and I obtained some relief from the application of warm flannels; but they always left me languid and relaxed.

On the fourth attack, I consulted a physician, who imagined that my complaint had been induced by drinking cider, in which I had formerly indulged. He ordered me weak Hollands and water for common drink, and prescribed the lixivium of tartar to be taken in broth. This medicine was persevered in for some time; but I found it gradually weaken my stomach, and impair my digestive powers.

About nine months after my first attack in the kidney I walked from Hampstead to London after dinner; and on the following day I clearly felt something pass from the kidney to the bladder, and suspected what it was. I took about a pint of Hollands and water; and on attempting shortly afterwards to void my urine, found that the passage was blocked up; but had scarcely time to consider of my situation, before the obstruction moved forwards to within

an inch of the extremity of the urethra. It remained there till the following evening; when, by the help of a small pair of watchmaker's forceps, I succeeded in extracting a stone, which was the source of the mischief. It was jagged and rough, and of a deep brick-red colour. I afterwards voided a considerable quantity of red crystalline sand.

My physician, who was apprehensive of a return of the disorder, desired me to purchase of Cadell an anonymous pamphlet upon the Stone and Gravel, and to observe the rules there laid down. This treatise particularly recommended the use of the alkalies. I therefore took the lixivium, and two bottles of Perry's solvent; but the red deposit in my urine continued; my loins felt weak, and when in bed very painful.

Being in the profession of the law, and much employed, I was under the necessity of leading a very sedentary life, which so aggravated my tendency to bile and indigestion, that I seldom could get above two or three hours' sleep.

With a view to alleviate these symptoms, and not with any idea of its being beneficial to the stone, I resorted to magnesia, which I continued with little intermission for eight months, in the dose of a tea-spoonful or two, every evening before I went to bed. The long vacation coming on, I gradually took more exercise, and used the cold bath. The tone of my stomach, at the end of the period I have mentioned, was so far restored as to induce me to set medicines of all kinds aside, except when any food or drink disagrees, when I occasionally resort to the magnesia. Under such treatment, the weakness and pain in my kidney left me, and the red sand entirely disappeared. I have

since enjoyed a very good state of health; and am now in my fifty-seventh year.

If I occasionally make a little free with the good things of this world, my stomach reminds me of the improper use of the lixivium, especially when I am prevented taking my usual exercise.'

The above case is important, not only as furnishing a striking and unprejudiced instance of the effect of magnesia in counteracting the tendency to form uric calculi and gravel, but likewise as demonstrating its efficacy where the alkalies had failed, and where the digestive organs had been injured in consequence of the use of such remedies. The time which had elapsed since the cure of this and other cases, without a relapse, is also strongly in favour of this mode of treatment.

CASE II.

A GENTLEMAN, twenty years of age, who had suffered from heartburn and other dyspeptic symptoms, was seized, on the first of June, 1811, with a violent pain in the loins, and more especially in the right kidney; and during the night he passed a large quantity of red sand with his urine. On the second, with a view to relieve the pain, which had increased considerably, he took fifty drops of laudanum, and drank freely of barley-water. The night was passed more quietly; but on the morning of the third he was seized with a violent pain in the kidney, and with the usual symptoms

of the passage of a calculus along the ureter. These continued with more or less violence until the evening of the 4th, when he became perfectly easy, and remained so till the morning of the 6th, when, with considerable pain and difficulty, he voided a calculus, composed of uric acid, weighing nine grains. For several successive days his urine deposited a large quantity of red sand, and three very small round calculi were voided.

He was now directed to abstain from all kinds of fermented liquors and sour food, and to take a pint of treble soda-water, containing three drachms of subcarbonate of soda, daily. Under this treatment he continued to recover, and remained perfectly free from complaint until the end of August, when a copious deposit of red sand appeared in his urine. He had little pain in the affected kidney; but complained of almost constant nausea or want of appetite. The soda-water was increased to a pint and a half, and afterwards to two pints daily; and in the intervals he drank very freely of barley-water.

Having persevered in this way for ten days without receiving any benefit, he was induced to make a trial of magnesia, of which he took one tea-spoonful, night and morning, in cold chamomile tea. In about a week the state of his stomach was much improved, and the deposit in the urine proportionably diminished: and in three weeks every symptom of disease had disappeared.

In February, 1812, having persevered in the use of magnesia with very little intermission, I was informed that the sand had returned; that increasing the quantity of magnesia had produced no good effect; and that alkalies

materially aggravated his complaint, by disagreeing with the stomach; and greatly increasing the urinary deposit.

On examining the sand, I found that, instead of consisting, as formerly, of uric acid, it was composed of a mixture of the ammoniaco-magnesian phosphate, with phosphate of lime. He was directed to abstain from magnesia and alkalies, and to adopt a plan of treatment which it is the object of the second Division of this paper more particularly to explain.

The foregoing is a well marked case of uric gravel, with a strong tendency to form calculi, materially relieved by the use of alkaline remedies. It illustrates their usual effects when carelessly persevered in, and shows the advantage with which magnesia may in such instances be employed: it also exhibits the effect of magnesia and the alkalies by producing the deposit of *white sand* (or phosphates) in the urine, when the *red sand* (or uric acid) has been removed.

The cases which follow are selected from among others, to explain the best mode of preventing the formation of white sand, and to show the most effectual treatment, where it is a natural deposit in the urine, or where it has been induced by the incautious exhibition of alkaline medicines.

Second Division.

The white sand so frequently voided by persons labouring under calculous complaints, was first analysed by Dr. Wollaston *, who found it composed of ammoniaco-magnesian phosphate, either alone or mixed with variable proportions of phosphate of lime. The use of acid medicines in these cases was also first suggested by the same able chemist; but although his valuable observations have been before the public for nearly fifteen years, I am not aware that any experiments have been made to ascertain what acids are best calculated to produce the desired effect, or to illustrate their mode of action.

Since my former communication, I have had no opportunity of attending to the important subject, and hope that the conclusions suggested by the following cases will be deemed satisfactory, and that their application in practice may lead to useful results.

CASE I.

A gentleman, fifty years of age, who about ten years before had undergone the operation for the stone †, was

* Philos. Trans. 1797.

† The stone extracted consisted of a nucleus of uric acid, about the size of a pea, incrusted with a mixture of the phosphates. It was broken during the operation, but appeared to have been of the size of a pigeon's egg.

attacked, on the 14th of January 1810, with violent pain in the right kidney and ureter, which lasted two days. On the 17th these symptoms subsided, and were followed by those of stone in the bladder, which continued for some days; and although he had taken abundance of barley-water, and similar diluents, the stone showed no disposition to pass. On account of his former sufferings, this circumstance rendered him extremely uneasy; and on the evening of the 21st, he suffered several severe paroxysms of pain on attempting to make water. Under these circumstances, he was desired to take a purge, composed of two ounces of infusion of senna, two drachms of tincture of senna, and twenty grains of powdered jalap.* In three hours this began to take powerful effect, and during the violence of the operation, he was so fortunate as to void the calculus with his urine: it weighed eight grains. On the 28th he again suffered pain in the region of the kidneys, and voided much sand, composed of uric acid, with ammoniaco-magnesian phosphate. He now took three half-pints of soda-water daily, which materially increased the proportion of the triple phosphate, while that of uric acid was considerably diminished. Ten drops of muriatic acid were then taken three times a day in water. The red sand now began to re-appear, and on the fourth of February he voided a very small uric calculus. The urine made

* I recommended this treatment, in consequenee of having heard Sir Everard Home state a ease, in his Surgieal Leetures, of a gentleman who suffered a bougie to pass so far into the urethra, that it eould not be removed by any instrument. During the operation of a purge of this kind, it was expelled with eonsiderable foree.

after dinner contained more or less mucus streaked with blood, a symptom which was much aggravated by a slight excess in wine. On the 6th he left London, and employed no medicine until the 12th, when he returned, in consequence of having voided a large quantity of white sand.

Having observed the efficacy of carbonic acid in preventing the deposition of the phosphates, and having found it less liable than any other acid to induce a return of the uric gravel and calculi, I now directed him to take half a pint of water, highly impregnated with fixed air, four or five times a day, and to drink cider instead of wine. On the 18th of February his urine was less turbid than it had been for some months before, and on the 20th of March, having continued the use of carbonic acid, he had no remaining symptoms.*

In August his urine became again turbid; but by the use of vinegar and lemon juice at his meals, which acids, he now finds, have no tendency to induce a return of the red gravel, he succeeds in preventing this symptom.

CASE II.

On the 11th of October 1812, the operation for stone in the bladder was performed upon a boy eleven years of age,

* I have several times examined the urine, with a view to ascertain whether any of the uric acid which was exhibited could be detected in the secretion; but the results of such experiments are so much interfered with by the very compound nature of the urine, that I have not hitherto been able to draw any satisfactory conclusions respecting them.

and four calculi were extracted, of which the largest was of the size of a small horse-bean. They were each composed of a nucleus of uric acid, upon which the ammoniaco-magnesian phosphate was deposited.

After the operation, the urine deposited a large quantity of white sediment, and some small pieces of red gravel were occasionally voided. He was now directed to take eight grains of citric acid, dissolved in barley-water, three times daily. Under this treatment the sediment in the urine was considerably diminished, but did not wholly disappear. The dose of the acid was gradually increased to twenty grains, by which means the sediment was only occasionally deposited, and consisted of little else than mucus. It was observed that whenever the citric acid was omitted, even for twenty-four hours, the sediment was greatly increased; and this was constantly attended with frequent desire to make water, and other symptoms of irritation in the bladder. On resuming the use of the citric acid, the sediment always disappeared, and the irritation of the bladder subsided; and this happened so frequently, that no doubt could be entertained of the influence of the medicine on the composition of the urine.

This plan of treatment was continued for three months. At the end of that period, it was found that the urine had not the same disposition to deposit the phosphates as formerly; even when the medicine was omitted, the sediment was small in quantity, and not constant in its appearance. He was now directed to omit the use of the citric acid, and occasionally to eat oranges and other acid fruits. He

continued this plan until the beginning of April 1813: his urine was then quite clear, and he had no symptoms of disease.

CASE III.

In the month of October 1811, a gentleman, thirty-four years of age, informed me that he had observed a white deposit in his urine during the whole of the preceding summer. He had taken considerable quantities of soda-water, which he thought increased the sediment; and alkalies in any other form produced a very obvious aggravation of the complaint.

His urine was at all times clear when voided; but after a few hours, a white powder was observed to separate from it, and a film of crystalline matter formed upon the surface.

The former consisted of phosphate of lime and mucus; the latter of the ammoniaco-magnesian phosphate.

He was directed to take one drachm of muriatic acid, properly diluted, at divided times during the day; and it was proposed that he should pursue this plan for a week; but it was discontinued on the third day, on account of its acting upon the bowels, and producing a frequent desire to make water.*

On the 10th of October he was advised to take two large

* In this, and other instances, the sulphuric and nitric acids were occasionally substituted for the muriatic; but they were found equally inadmissible.

glasses of lemonade daily, and to substitute claret for port wine, a pint of which he was in the habit of drinking daily. Under this treatment the symptoms produced by the muriatic acid subsided; but the appearance of the urine was not at first improved.

On the 20th the film of triple phosphate, formerly observed constantly in the urine, began to decrease; but the white sand remained as abundant as before: he was, therefore, directed to take twenty grains of citric acid twice a day, and to continue the use of acid drink as formerly.

The additional acid at first disagreed with the bowels; but this effect soon ceased, and the sediment was only observed in the urine voided in the morning: he therefore took another dose of the acid every night. This plan was pursued with little intermission until the beginning of December; the deposition of the phosphates gradually ceased, and he remained in perfect health until the middle of May, 1812, when, after violent exercise and taking more wine than usual, the white sand again made its appearance in great abundance; his stomach became extremely irritable, and the acids which he had before employed with success brought on considerable irritation in the bladder. The addition of ten drops of laudanum to each dose of the citric acid prevented this effect; and he was thus enabled to continue the acid, which in a fortnight relieved his complaint.

This gentleman informed me, that whenever he omitted the use of an acid diet, or took much wine, especially port, his urine deposited the white sand and mucus for two or three days.

CASE IV.

A GENTLEMAN, eighty years of age, who had twice submitted to the operation for the stone within five years, voided with his urine considerable quantities of white sand and mucus.

From the age of this patient, and the account of his case, there appeared little doubt that the calculi had been formed in consequence of a diseased prostate gland, in the manner described by Sir Everard Home *; and on examining them, they were found to contain no uric nucleus, nor indeed had there been any symptoms of disease in the kidneys at any previous period.

This gentleman had been in the habit of taking soda-water, from which he was now desired to abstain, with a view of putting him on the acid mode of treatment. He was ordered to take eight drops of muriatic acid three times a day in two table-spoonfuls of water; but the third dose produced so much irritation in the bladder, and consequent increase of his symptoms, that it became necessary to adopt another treatment.

Lemon juice, or a solution of the pure citric acid, when given in quantity sufficient to produce any change in the appearance of the urine, had the same effect as the muriatic acid.

* Practical Observations on the Treatment of Diseases of the Prostate Gland, p. 39.

As water impregnated with carbonic acid could not be procured, he was directed to dissolve, in separate portions of water, twenty grains of citric acid, and thirty grains of the crystallized carbonate of potash, and to take the mixed solutions during the effervescence. This quantity was at first only taken night and morning; but as it agreed perfectly well, it was afterwards repeated four and five times daily. Under these circumstances, the appearance of the urine was soon improved, and both the mucus and the sand were considerably diminished in quantity. In six weeks the urine, when voided, was transparent; but a considerable deposition of the phosphates took place when it had remained for some hours at rest. In this state he left London; and has since informed me, that the sediment gradually diminished under the use of the carbonic acid, that his urine is never turbid, and that the irritation in the bladder has entirely subsided.

It did not appear necessary to detail the minutiæ of the above cases: they have been selected with a view to elucidate the treatment of the disease, as far as it depends upon chemical principles, and to furnish the data upon which the following conclusions are founded: —

1. That where alkalies fail to relieve the increased secretion of the uric acid, and to prevent its forming calculi in the kidneys, or where they disagree with the stomach, magnesia is generally effectual; and that it may be

persevered in for a considerable time without inconvenience where the tendency to form excess of uric acid remains.

2. When the alkalies, or magnesia, are improperly continued, after having relieved the symptoms connected with the formation of the red sand, or uric acid, the urine acquires a tendency to deposit the white sand, consisting of the ammoniaco-magnesian phosphate, and phosphate of lime.

3. The mineral acids (muriatic, sulphuric, and nitric) diminish or entirely prevent the deposition of the phosphates, but are apt to induce a return of the red gravel.

4. That vegetable acids, especially the citric and tartaric, are less liable to produce the last-mentioned effects, even when taken in large doses for a long time; and that carbonic acid is particularly useful in cases where the irritable state of the bladder prevents the exhibition of other remedies."

SECT. VII.

On Earthy Concretions voided from the Bowels.

It is by no means uncommon, when the human stomach has become deranged in its functions, that a preternatural accumulation of viscid mucus shall take place in its cavity. A similar disposition occasionally occurs in every part of the intestinal canal. In this state of the stomach and intestines, the bowel has a complete covering of compact mucus spread over its surface; which, when it is voided per anum, has so near a resemblance to the gut, of which it is a cast, as frequently to have been mistaken for a portion of the intestine.

Cases of another kind, though of more rare occurrence, have been met with, in which the earthy salts have been entangled in this mucus; and, crystalizing there, have formed concretions, not unlike those of which the nucleus has been a prune stone, or the husk of a grain of oats, accounts of which are recorded in the Philosophical Transactions. This appears to have happened in the following case: A lady, twenty-eight years of age, whose health had for many years been delicate, was liable to attacks of indigestion, irregular action in the bowels, and sometimes they were wholly obstructed. When twenty-four years old,

she was attacked with spasms in the region of the cæcum, attended with inflammation, which was confined to that part, and produced a complete constipation. Large doses of calomel, and copious bleeding, carried off these symptoms, and she was relieved by the evacuation of a pint in measure of what was considered to be fragments of gall-stones, mixed up in a viscid mucus. What passed from the bowels was, from that period, more or less loaded with the same kind of mucus, and a course of mercurial purgatives was persevered in. This plan produced weakness and irritability. Although it kept under the symptoms to a certain degree, it did not prevent accumulations of mucus once every three months; and the attacks were so violent in their effect upon her general health, that she was considered upon every recurrence to be in imminent danger of her life. It was under these circumstances that I was consulted in the latter part of 1821. I denied the probability of what had produced the original accumulation having been gall-stones, or that calomel was the only medicine capable of removing the symptoms; but said the first thing to be done was, to have the mucus analysed, which would enable us to discover the best mode of preventing its formation. For this purpose I applied to my friend Mr. Children, in preference to any other member of the Society for promoting Animal Chemistry, as he had analysed earthy concretions found in the colon of a boy who had swallowed plum-stones, and these not passing through, had become encrusted. His analysis is published in the volume of the Philosophical

Transactions for the year 1822. He analysed eleven different portions of this mucus, the first voided December 2d, 1821, the last the end of January 1822.

The following is the result of his examinations: The mode of examination adopted was to filter a portion of the contents of the phial: it gave no precipitate with corrosive sublimate nor infusion of galls, but an abundant one with subacetate of lead. Boiling produced no coagulum on another portion. When filtered and tested by the same reagents, it gave similar results. The animal matter, therefore, appears to be mucus; but some of the most firmly inspissated did not dissolve in hot or cold water, and was little altered in its appearance by digestion in either.

The whole quantity of solid matter contained in each phial was ascertained by evaporation to dryness by steam. To obtain the saline substances the residuum was incinerated, and the soluble salts dissolved by distilled water. The insoluble portion was digested in oxalic acid, to separate the ammoniaco-magnesian phosphate, which was thrown down again by ammonia. The remainder was dissolved in muriatic acid, and its nature as well as that of the soluble salts examined by the usual tests.

It appears that the substance of the discharge consisted chiefly of inspissated mucus, with a small portion of common salt and phosphate of lime, and occasionally some other saline substances.

The result of a second experiment, on the 16th of December 1821, gave for six fluid ounces eight and a half grains of solid matter, which consisted of five grains of

mucus, and half a grain of saline matter, principally phosphate of lime, with a little common salt and sulphate of soda.

January 5th, 1822, a similar quantity gave 12·56 grains of solid matter which contained

10·09 mucus.

2·47 common salt, with a portion of carbonate of soda and a little phosphate of lime.

12·56

After the animal matter was burnt away, the water in which the residuum was digested was strongly alkaline.

January 10th, a similar quantity gave

6·1 grains of solid matter.

0·93 grains of saline matter, chiefly phosphate of lime with a slight trace of ammoniaco-magnesian phosphate, common salt, and sulphate and carbonate of soda.

The animal matter as in every case mucus.

January 15th, six fluid ounces gave

6·4 grains of solid matter.

0·58 grains of saline, chiefly soluble salts, viz., sulphate, muriate, and carbonate of soda, with a very minute trace of carbonate of lime.

January 27th, six fluid ounces gave

6·3 grains of solid matter.

0·4 grains of ashes, almost wholly phosphate of lime, with a trace of common salt.

January 28th, six fluid ounces gave

27·45 grains of solid matter.

3·37 grains of ashes, containing a small portion of soluble salts, viz., carbonate of soda, a little sulphate of soda, and about 0·4 of a grain of common salt. The insoluble portion was chiefly phosphate of lime, with a very little ammoniaco-magnesian phosphate.

The symptoms this inspissated mucus produced were sickness, debility, loss of appetite, and the substance was found to be gritty, and gave excruciating pain in the expulsion, similar to cutting with a knife.

Before the analysis was made I gave a full trial to the use of gum olibanum, aloes, guaiacum, and gamboge, without producing any diminution in the quantity of mucus generated.

I succeeded in keeping the bowels open by means of the compound decoction of aloes in large doses; and cayenne pepper relieved, in some degree, the pain in the stomach.

Under these circumstances, I stated the case at a meeting of the members of the Society for promoting Animal Chemistry, along with the result of the analysis. Sir Humphrey Davy suggested the internal use of the tannin, as the medicine most likely to dissolve the mucus when formed, as also to prevent its being generated.

Under the use of this remedy, the mucus diminished in quantity, the pain in passing it went off: in the course of six months she had no return of the attacks; but if she omitted the medicine, her stomach had an uncomfortable feel, which went off when it was resumed. She persevered

in taking the tannin for more than a year, and then as a substitute took occasionally the compound decoction of aloes, and pills of cayenne pepper and ipecacuanha.

She began the tannin the 14th of January 1822, left it off entirely in 1824, and has had no return of her complaint in 1827; but is still obliged very often to have recourse to the decoction and the pills, which she does whenever the mucus is voided in any quantity at any one time.

SECT. VIII.

On the Action of Medicines through the Medium of the Circulation, and the Effects of Vinum Colchici on Gout.

A KNOWLEDGE of the readiness with which liquids pass from the stomach into the circulation, carrying along with them the impregnation of different medicines, and the readiness with which such medicines are carried from the circulating blood, by the action of the kidneys, led Mr. Brande and myself to an enquiry respecting the prevention of gravel and gout.

The action of different substances on the contents of the stomach, and those most efficient in depriving them of the principal ingredient met with in stone and gout, have been already detailed.

For the cure of the gout, the eau médicinale of Husson has been most fortunately discovered to be a specific remedy; and I shall prove that it is now ascertained, by experiments on different people, that a vinous infusion of the colchicum autumnale, or meadow saffron, is equally so, and, therefore, the two medicines must be considered as the same.

To ascertain the mode of action of the colchicum, appeared to me an enquiry connected with the objects of animal chemistry, which are not confined to the knowledge

of purely chemical combinations in the stomach, or other parts of the body, but include the effects of galvanism on the nerves, and of mineral and vegetable solutions on the blood, so far as they affect the actions of life, or the symptoms of disease.

It has already been determined by experiment, that almost every mineral, vegetable, and animal poison, if not the whole of them, is carried into the circulation before it produces its specific effects upon particular parts, whether these are the stomach, skin, or other parts of the body. The most truly specific medicine that we have been hitherto acquainted with is mercury for the venereal disease; and it is completely established, that this remedy, when in the circulation, is equally efficient in the cure of a recent chancre produced by inoculation, and a venereal sore throat, in consequence of the disease having been carried into the circulation.

That other medicines can be received into the circulation, and as soon as they arrive there, produce their effects upon different parts of the body, is proved by experiments made by the late Mr. Hunter, although he had no idea of their being usually carried there before they produce the different actions so well known to follow their exhibition by the mouth. He found that infusion of the following substances received into the circulation by the jugular vein, immediately produced the same effects which more slowly follow their being taken by the mouth. Infusion of opium brought on drowsiness. Infusion of ipecacuanha, vomiting. Jalap, vomiting and purging. Infusion of rhubarb, a profuse flow of urine. These effects ceased in a

few hours, and appeared to have in no respect injured the animal's health. Except the venereal disease, gout is the only one whose local symptoms have been completely removed by medicine in so short a time as to put it beyond all doubt that their removal is the effect of the medicine. The effect of the eau médicinale, and of the vinous infusion of the colchicum autumnale on gout, is indeed more rapid than that of mercury on the venereal disease, but in all other respects corresponds with it; and of these medicines, the only difference may be, that the one is more quickly received into the circulation than the other.

This power of the eau médicinale, which I have stated to be exactly similar to that of the colchicum autumnale, over the local symptoms of gout, I have ascertained by experiment, more than six times upon myself; at one time the symptoms went off in six hours, at another in twelve, and at others in twenty-four hours.

As we know the sensible effects of mercury, whether it is introduced into the circulation, or received into the stomach, are the same, we conclude, whenever these sensible effects are met with, that mercury is actually in the circulation.

It therefore occurred to me, that if the infusion of the colchicum should prove to be the same, whether it is introduced into the circulation by the jugular vein, or received by the mouth into the stomach, that we might equally in both cases conclude it to be in the circulation. To determine this point, thirty drops of the vinous infusion of the colchicum (made by macerating two pounds of the fresh roots in twenty-four ounces of sherry wine, in a gentle

heat for six days, the spirit being previously carried off by heat) was diluted with a dram of water, and conveyed into the circulation of a moderately-sized dog, by the jugular vein. The dog's pulse in a natural state is 140 in a minute.

In five minutes the dog had a tremulous motion of the muscles, and fluttering of the pulse, accompanied with nausea, but no retching to vomit. In fourteen minutes the pulse was 180 in a minute, and had frequent intermissions. In four hours the pulse was 120 in a minute, of its natural strength, and had frequent intermissions. In seven hours the dog had a natural motion; the pulse had no intermission, was 140 in a minute. The dog had a good appetite for food, and appeared in perfect health.

The same dog, at the end of three complete days, swallowed sixty drops of the same infusion, exactly double the quantity that had been introduced into the circulation. In two hours he became languid, the pulse wiry and weak, but 140 in the minute. In four hours and a half the languor became much less, and the pulse natural. In eight hours the dog had a natural motion. In eleven hours was in good spirits and very well.

The sensible effects upon the dog were similar to those produced upon myself, but in a less degree. Under the influence of a violent fit of the gout, in the ankle, on the 23d of December 1815, at ten o'clock in the morning, I took sixty drops of eau médicinale. The pain of the gout was insufferable. I got into bed, and was so chilly as not to be able to keep my hands warm, even under the bed-clothes. In two hours I became rather hot and thirsty.

In three hours the pain was so much diminished as to be tolerable, while the limb was at rest. In seven hours I had a confined motion from the bowels, and the pain in the ankle became severe while the foot was placed on the ground, but this went off as soon as the foot was again placed in a horizontal posture. A nausea or half sickness came on; my pulse, which is naturally eighty in a minute, was lowered to sixty, and intermitted. In ten hours the nausea was gone off, but I remained languid, the pulse beating seventy in a minute. I had some appetite for food.

The following morning my pulse was eighty, and having passed a good night, I was enabled to walk as usual, and follow the duties of my profession.

If these observations shall be confirmed, they must lead us to conclude, that the different kinds of substances, which produce specific diseases, are first carried into the circulation, in the same manner as mineral and animal poisons, and that the medicines by which they are acted upon go through the same course, before they produce their beneficial effects; a material step will thus be gained in the consideration of diseases, and the modes of treating them.

Having laid before the Royal Society the remarks I have just detailed, I was anxious to establish what appeared to me two important facts; one, that the infusion of the

colchicum can be received into the circulation without producing any permanent mischief; the other, that it is through the medium of the circulation its beneficial effects upon gout are produced, and, therefore, the sudden relief which is experienced can be readily explained. Having attended to the effects of this medicine for several years, in cases of gout, both in my own case, and in those of my friends, I found invariably that it diminished the frequency of the pulse ten or twenty beats in a minute, and this effect generally took place about twelve hours after the medicine was exhibited. I therefore considered this to be the criterion of the constitution being under the influence of the medicine; and when I found that the pulse was affected in the same way by the medicine received into the circulation, and in a much shorter time, I became satisfied that in both cases this arose from an effect upon the circulation, and not upon the stomach, and therefore did not prosecute the enquiry; since exhibiting larger doses could only confirm what is already known, namely, that the medicine is capable, when injudiciously used, of producing very violent effects.

It was suggested to me, that the only mode of proving that the medicine acts through the medium of the circulation, is to show that when a sufficient quantity is received into the blood, all the violent effects are produced that result from a large dose taken by the mouth; and as I had no object but the pursuit of truth, I lost no time in complying with this suggestion, and introduced into the circulation of a dog 160 drops of the same infusion before employed.

The animal lost all power of voluntary motion, the breathing became extremely slow, and the pulse was hardly to be felt. In ten minutes the pulse was eighty-four, the inspirations were natural, which are forty in a minute. In twenty minutes the pulse was sixty, the inspirations thirty in a minute; a tremulous motion had taken place in the hind legs. In an hour the pulse was 115, and irregular; the animal was capable of sitting up, but was in a state of violent tremor, and the inspirations could not be counted.

In one hour and a half, the tremor had gone off, the pulse continued the same; the animal made ineffectual attempts to vomit, and continued to do so for ten minutes, accompanied with great languor; the inspirations were fifty-four in a minute.

In two hours the pulse was 150, and very weak; the animal had voided an ounce and a half of water, had vomited twice, each time bringing up a quantity of mucus tinged with bile, and had two liquid stools.

In three hours had vomited again, and had two liquid stools.

In four hours continued extremely languid.

In five hours vomited some bloody mucus, and expired.

On opening the body, the stomach contained mucus tinged with blood, and its internal membrane was inflamed; the duodenum had its internal surface universally inflamed; the same appearance in a less degree was met with in the jejunum and ilium, and more strongly marked in the colon than the ilium.

The facts now adduced afford sufficient proof of the action of the colchicum autumnale upon the different parts

of the body being through the medium of the circulation, and not in consequence of the immediate effects upon the stomach and intestines.

Having found that the infusion, made as above directed, after standing some months, threw down a deposit, the removal of which in no respect diminishes the efficacy of its medicinal effects upon gout, and renders the infusion less irritating to the stomach and intestinal canal, I am desirous of laying these new facts before the public.

The bulb of the colchicum autumnale contains a certain quantity of extractive matter, and a large portion of mucilage, both of which are taken up by the wine, in the first instance, and when the strained liquor is allowed to stand, a considerable deposit almost immediately takes place.

In the first trials that were made with this medicine in St. George's Hospital, it was natural to enquire whether this deposit contained any medical virtues, and upon trials frequently repeated it was found to have none.

This led to the opinion that the extractive matter suspended in the wine was alone the active part of the medicine, and not only the first deposit, but also that which from time to time was afterwards found to take place, was inert.

Of this opinion I was led to entertain considerable doubts, in consequence of having found, upon one occasion, in which I took half a bottle of the eau médicinale, which had been poured off without shaking the bottle, that the sensible effects were very mild; those produced by the

other half, in which the deposit was mixed, were unusually severe, the nausea being greater, and a greater number of stools being produced.

These doubts were much strengthened when I found that the effects of the eau médicinale are more violent, upon many stomachs, than those of the vinous infusion of the colchicum, which probably arises from the eau médicinale being kept in small bottles, consequently all the deposit is given; but the infusion, being kept in large bottles, the deposit falls to the bottom. If such deposit increased the powers of the medicine in counteracting the symptoms of gout, it would be unnecessary to prosecute this investigation farther, since it would be absurd to diminish the violence of a medicine, if, by so doing, its efficacy is to be diminished in an equal degree.

To ascertain this point, I gave sixty drops of the vinous infusion of colchicum, in which there was no deposit whatever, to a man labouring under a severe paroxysm of gout, to which he was a great martyr, and whose paroxysms were usually of several weeks' continuance; he was sixty years of age.

The medicine was exhibited on the 17th of January 1817, his pulse being 115. In half an hour he had a slight nausea, which soon went off. In five hours a profuse perspiration came on, and the pain of the gout entirely subsided, leaving a soreness in the parts that had been affected. In twelve hours the bowels were gently moved, his pulse 105, and irregular; in fourteen hours his bowels were acted on a second time; in nineteen hours his pulse was ninety-two, and natural; in forty-eight hours he was

quite well, and has continued so for a period of more than three months.

The result of this case satisfied me, that the infusion contained the specific remedy for the gout, and that the deposit is not necessary for its removal.

This rendered it probable that, where the deposit is taken along with the infusion, its solid form prevents it from being carried into the circulation of the blood, and it remains in the stomach, producing more or less mischief in that viscus, without being any way concerned in driving away the symptoms for which the medicine was exhibited; in this respect resembling many of the salts of mercury, which irritate the bowels, without relieving the symptoms of the venereal disease.

I explained these opinions to Mr. Gatcombe, who at that time gave me his assistance in my professional pursuits, and requested him to investigate this subject.

To do this more completely, he began by repeating the three experiments detailed in the first part of these observations, substituting the eau médicinale for the vinous infusion of colchicum, so as to determine with more precision, whether they are or are not the same medicine.

Experiment I.

Thirty drops of the eau médicinale, with the deposit, were injected into the jugular vein of a dog: the effects were the same as in my experiment with the same quantity of

the vinous infusion of colchicum, only the animal was two hours longer in recovering from them, and was purged for nine hours afterwards.

Experiment II.

Sixty drops of the eau médicinale were given by the mouth to the same dog; the effect was less than in my experiment with the vinous infusion of colchicum exhibited in the same quantity: this arose from a very copious evacuation of urine having been produced.

Experiment III.

One hundred and sixteen drops of the eau médicinale, injected into the jugular vein of a dog, produced rather more violent effects than in my experiment with the same quantity of the vinous infusion of colchicum: the animal died in six hours; and after death the effects of inflammation in the bowels were more violent, approaching to mortification.

Mr. Gatcombe having found so exact a similarity in the effects of the two medicines, in these trials, I requested him to make the following comparative experiment, on the effects produced upon the stomach and bowels by the eau médicinale, in which there is a deposit, and the vinous infusion of colchicum, in which there is none.

Experiment IV.

One hundred and sixty drops of the eau médicinale, taken by the mouth, produced the same effects, and left the same appearances after death, as when that quantity was injected into the vein, only the animal lived nine instead of six hours.

One hundred and sixty drops of the vinous infusion of colchicum were given to a puppy of the same litter: they produced vomiting, purging, and a great flow of urine; but the animal very soon recovered.

Two hundred drops of the same infusion, after an interval of several days, were given to the same dog, and the effects were the same; the dog had become much improved in his looks and condition.

Three hundred drops, after an interval of several days, were given to the same dog: effects corresponding with those of 160 drops of the eau médicinale were produced. The dog died in nine hours, and the appearances of inflammation after death were of the same kind, but not nearly so extensive.

From these experiments the eau médicinale with the deposit produces double the irritation on the coats of the stomach and intestines that is brought on by the vinous

infusion of colchicum: this probably arises from the local inflammation brought on by the deposit upon the internal membrane of these viscera.

To determine as nearly as possible the effects of the deposit, when applied in a solid form to the coats of the stomach and intestines, the following experiment was made:—

Experiment V.

Six grains of the deposit of the vinous infusion of colchicum were given to a dog in bread and milk: in three hours it produced vomiting and purging, which lasted twenty-four hours. During the latter part of that time there was blood in the stools, as well as in what was brought up from the stomach.

I wished to repeat this experiment, with the deposit from the eau médicinale, but found in bottles that had been kept seven years the wine had become vapid, and in this decomposed state the acrid part of the deposit had been taken up again; so that in twelve bottles, containing different quantities, only five grains could be procured which was quite inert.

Being at a loss to know, whether the extractive matter, deposited from the infusion, is in reality more acrid to the stomach than that suspended in it, or the circumstance of its being applied in a solid form renders it so, I requested

Professor Brande to acquaint me, if it could be the effect of any chemical decomposition having taken place.

He favoured me with the following explanation, which is highly satisfactory: —

"There are certain vegetable bodies which, when infused in water, or diluted spirit, furnish a solution which lets fall a sediment in which their activity, as purgative medicines, chiefly resides: this is remarkably the case with the wild cucumber or elaterium. The sediment is a very drastic purge: the part that remains dissolved is comparatively mild in its operation upon the bowels."

This explanation of Professor Brande applies to the colchicum; and we are now enabled to separate the purgative qualities of the vinous infusion of colchicum and eau médicinale from those which prove a specific for the gout, in the simplest possible manner, by keeping them in large bottles instead of small ones, and not going too near the bottom.

It also explains what is asserted by Professor Alpinus *, that the Egyptian women eat the fresh bulbs that they may grow fat; an effect which was found to take place in the dog, while the dose was confined within such limits as not to act too violently upon the bowels.

The bulbs of the Egyptian colchicum, when long kept, weigh one drachm each: on being steeped in water they double their weight; so that the quantity of extractive matter contained in two or three recent bulbs, while combined with the mucilaginous matter, of which the bulbs

* Hist. Nat. Egypt. Pars i. lib. iii. cap. 14.

are principally composed, is not likely to be sufficient to do more than act as a brisk purgative, the occasional use of which tends to make people grow fat.

Since these experiments were made, the patient who is mentioned as having had the gout in January, has had another attack: it came on upon the 10th of July, and was removed in the same manner as the former, by the same dose of the medicine. The President of the Royal Society, Sir Joseph Banks, convinced by the evidence contained in the former pages, that the vinum colchici, in which there is no deposit, must be a less hurtful medicine than the eau médicinale, thought it a duty to himself and the public to make trial of it; and on the 10th of July, when the gout in his left hand, and in the whole of the joints of that side of the body, was very severe, allowed me to give him ninety drops of the vinum colchici, and found that the symptoms of gout were sooner and more completely removed than they ever had been by the eau médicinale, of which he had an experience of seven years, having taken it regularly ever since the 17th of February, 1810, and during that time kept a regular account of the doses, their effects, and the intervals between them. Sir Joseph Banks continued the use of the vinum colchici from this time to his death, and sixty minims always succeeded in carrying off the fit.

In the year 1816, when fifty-nine years of age, having suffered for ten years very severely from gout, which in that period was every year becoming more and more severe, the attacks more violent, the fits of longer continuance, and the consequent debility of greater duration, I was induced to

adopt the use of the vinum colchici prepared according to the directions Sir Joseph Banks gave upon the subject, making the infusion at a time when the bulb was in the highest perfection, which is at the latter end of August or beginning of September: it is to be digested for a week, then pressed. I found sixty minims invariably removed the fit, and, what is highly satisfactory, after a trial of eleven years I have never increased the dose, nor repeated it for any one attack. I have not any one joint, at the age of seventy-one, in which the ligaments are thickened, or any chalk deposited. The medicine in eight hours produces a mitigation of pain; in twelve hours the pain ceases, while the parts are not put in motion; and in sixteen hours I am able to pursue the duties of my profession. The medical effects are an unusual flow of pale urine during the first twelve hours, nausea, and a lax motion from the bowels. At the end of sixteen hours, or thereabouts, the urine is loaded with uric acid, which ceases in eight or ten hours. Whenever there is any deposit in the bottle, I have the liquor filtered.

CHAPTER II.

On the Blood.

Hervey discovered the circulation of the blood, and showed that this fluid is the material out of which the growth and repairs of animal bodies are derived.

This discovery was made in the reign of Charles the First. Hume, in his History of England, gives the following interesting account of Hervey :

" Hervey is entitled to the glory of having made by reasoning alone, without any mixture of accident, a capital discovery in one of the most important branches of science. He had also the happiness of establishing at once this theory on the most solid and convincing proofs; and posterity has added little to the arguments suggested by his industry and ingenuity. His treatise on the circulation of the blood is further embellished by that warmth and spirit which so naturally accompany the genius of invention. This great man was much favoured by Charles the First, who gave him the liberty of using all the deer in the royal forests, for perfecting his discoveries on the generation of animals. It was remarked that no physician in Europe, who had reached forty years of age, ever to the end of his life adopted Hervey's doctrine of the circulation of the blood, and that his practice in London diminished ex-

tremely from the reproach drawn upon him by that great and signal discovery. So slow is the progress of truth in every science, even when not opposed by faction or superstitious prejudices. He died in 1657, aged seventy-nine."

John Hunter took up the blood as the great first principle in his theory of animal life; but neither of these great men were chemists or microscopical observers, and without those acquirements they were unable to examine its separate parts.

SECT. I.

On the Component Parts of the Blood.

A MORE extensive knowledge respecting the nature and component parts of the blood has been the result of Mr. Bauer's microscopical observations made upon that fluid, in the present century, than had been obtained in any former period since the commencement of the study of anatomy.

By his examination we have been shown that it is made up of a number of distinct parts, many of which were not before known to exist in it; and I am led to believe that from this knowledge we are now enabled to allot to each of these the office it is to perform in the animal economy. With this view, I shall describe separately the different component parts of the blood in detail, and endeavour to point out the various structures in animal bodies which each of them is adapted to build up and keep in repair.

The fluid blood examined in the microscope, magnified forty times, is ascertained to consist of red globules suspended in a fluid called the serum, and when the blood remains at rest, it undergoes a decomposition. The red globules, which are $\frac{1}{1800}$ part of an inch in diameter, separate into globules of $\frac{1}{2000}$ part of an inch, colouring matter, and a gelatinous substance, which being transparent

is in common circumstances invisible, suspending globules $\frac{1}{5000}$ part of an inch, and a gas is evolved called carbonic acid gas.

The serum in its decomposition separates into lymph globules, mucus, and water: the lymph globules are from $\frac{1}{2800}$ to $\frac{1}{3400}$ parts of an inch in diameter. Having extracted from the blood these eight distinct ingredients, I shall now endeavour to point out the purposes to which each of them is applied in the living animal body, beginning with the most essential organs.

1. Globules, $\frac{1}{4000}$ part of an inch in diameter.

2. Transparent gelly, in which these globules are suspended, and which is soluble in water.

These in their combined state compose the essential parts of the brain, spinal marrow, ganglions, nerves, the male semen, the contents of the ovum in the female.

3. Colouring matter, defends the blood globules, while circulating in the living body, from coalescing, which they do as soon as deprived of it.

4. Blood globules: these form the muscular fibres.

5. The lymph globules, form the cellular tissue, ligament, and tendon.

6. Carbonic acid gas. This, pervading the coagulable lymph, forms all the tubular structures in animal bodies.

7. Water enters into the composition of all the organs of animal bodies.

8. Mucus, lubricates and defends the internal surface of all the outlets of the body.

Besides these essential component parts of the blood, many others are found in the circulation, as phosphate and

carbonate of lime, which are required in forming bone and shell; also essential oils, acids, alkalies, and neutral salts.

We have much yet to learn respecting the component parts of the blood, since even the principal distinction between the blood globules deprived of their colouring matter, and those of the lymph, which is their size, was unknown till the microscope was employed in the present investigations. Before, they were only known to be heavier; and this criterion could only be resorted to when the blood was in the act of decomposition, since, while that was going on, the blood globules from their weight sunk to the bottom of the vessel, and the lymph, whose globules had never been examined, remained suspended over them; but when the colouring matter was washed away, and the coagulum that remained was analysed chemically, the same results were produced from both the blood and lymph globules.

In proof of the observations I have made being correct, I got Mr. Brande to make a comparative analysis of the upper and under portion of a cup of very buffy blood taken from the arm, the colouring matter being washed out. He found them to give the same results, except that the upper portion contained less water. When examined by the microscope, the upper portion was found to consist of globules $\frac{1}{2800}$ part of an inch in diameter, the lower of globules $\frac{1}{2000}$ part, so that in future no mistake can be made between them.

This mode of distinction has led to the correction of the error of the two substances being the same, and has also en-

abled me to correct another that has been universally received, which is, that the buffy part of the coagulum is the most elastic, and the cupped appearance of buffy blood is the effect of its superior degree of contraction.

This last error was detected by observing that three cups of blood extracted from the arm had the upper surface not only buffy but quite flat, and where the buffy part joined the lower red portion of the coagulum, there was an hour-glass contraction. Upon trying the comparative elasticity of the two portions, the lower red coagulum was considerably more so than the coagulable lymph or buffy part, and it is this circumstance that prevents the coagulum of inflamed blood from taking on the form of a sphere. I shall give the particulars of the case : —

The blood was taken from a person thirty-five years old, who eight months before had a blow on the skull which was not followed by any bad effects, and therefore he went on pursuing his usual employments : but after this lapse of time, he became liable to fits, in which he was insensible, and when they went off left a considerable degree of head-ache which did not yield to any mode of treatment that was adopted. In this state he came under my care in St. George's Hospital. I had him bled repeatedly : the blood was very much cupped and buffy, and as the symptoms were not relieved, I continued every second day to take away nine ounces of blood, always in three separate cups, till in the course of thirteen days I had extracted sixty-eight ounces. The last two cups attracted my attention from the extraordinary circumstance of the surface of the crassametum, although quite as buffy as any of the former,

instead of being cupped was quite flat, and the outer edge, which extended beyond the body of the coagulum, was turned outwards. This appearance, unlike any thing that I ever had seen or heard of, made me put the coagulum into proof spirits for its preservation; the effect of the spirit, although it made the thin edge contract in a small degree, produced no other change, and enabled Mr. Bauer to make the annexed drawing, accurately representing the appearance it put on.

The malady which had produced so unusual an effect upon the blood circulating in the veins, was found, by an examination of the body after death, to be an inflammation of only one convolution of the right hemisphere of the cerebrum, at the most prominent part, where it is covered by the dura mater, just as the falx dips down between the two hemispheres.

When the globules contained in the upper portion of the coagulable lymph were measured by the micrometer in the microscope, they were $\frac{1}{2800}$ and $\frac{1}{3200}$ parts of an inch in diameter: the lower portion was made up of globules of $\frac{1}{2000}$ and $\frac{1}{2400}$ parts of an inch in diameter.

Professor Brande, who examined the chemical properties of these portions, informs me, " that the upper portion had the usual properties of the albumen of the blood. When carefully dried, it assumed the appearance and chemical characters of horn. The lower coloured portion was looser in texture, and considerably softer. In dilute acetic acid it soon became perfectly gelatinous, and was dissolved in it by the aid of gentle heat. It contained a much larger relative

proportion of water than the upper portion, and shrunk much more when dried, in which state, however, it resembled dried albumen. It appears that, independent of colouring matter, these two portions of coagulum differ chiefly from each other in their state of concentration.

SECT. II.

On the Coagulation of the Fluid Blood in an Aneurismal Tumour by means of Heat.

In a case of aneurism in the external iliac artery, in Chelsea Hospital, for the cure of which I tied the femoral artery below the sac on the 10th of September, 1825, upon finding that this operation neither diminished the pulsation nor arrested the increase of the tumour, I was led to introduce a needle, in the same manner as it is done in the use of the acu punctorium for rheumatism, into the centre of the swelling where the pulsation was most violent. The needle was passed through a small hole in a bar of steel three inches long, the skin of the thigh was guarded by cork, and the needle heated by a spirit lamp applied to the other end of the bar. In five minutes the patient felt heat and pain in the centre of the tumour, but not very severe, so that the application was readily endured for fifteen minutes, and during that time the pulsation in the tumour became less distinct. On withdrawing the needle; the orifice in the skin was marked by a single drop of coloured serum. In half an hour intense heat was felt in the thigh; but in ten minutes after taking twenty drops of laudanum it went off; the throbbing in the sac was reduced to an undulation, and the feeling of uneasiness in the thigh, which had for the last twelve hours been considerable, was much abated.

This application of heat was not made till the swelling of the tumour had increased considerably, and it was twenty-eight days after tying the femoral artery. The tumour continuing to increase, the operation was repeated after an interval of eight days, employing a needle and bar of steel double the size of those before used, and allowing them to remain thirty-five minutes. The heat felt was greater than in the first trial, and the pulsation lessened in a greater degree. The needle required force to withdraw it; and a coagulum, as hard as sealing-wax, the size of a small pea, was found surrounding the middle of the needle. The pain ceased in ten minutes, the internal heat continued twenty-four hours, and the tumour had acquired a solid feel. The parts were quite easy for two days. On the third the pulsation returned, the tumour became tense, and there was greater internal pain than had been before felt. After an interval of ten days the operation was repeated, the needle and bar of steel four times larger than those first used. The heat felt in the tumour was very great, and in twenty minutes the pulsation ceased; I therefore removed the needle, and the parts were quite easy. The tumour was solid to the feel; no pulsation could be distinguished nor pain felt, and the patient had a good appetite. The circulation in the thigh was so much diminished, that symptoms of mortification had begun in the foot, and the patient died before it actually had taken place, upon the seventieth day from that on which the artery was tied.

From the appearance of the parts after death, it was fully proved that coagulation in the aneurismal tumour had been effected by the degree of heat applied; however, the disease

had been allowed to advance too far before the mode of treatment had been resorted to. The appearances are shown in the annexed drawing.

It is a circumstance that could not have been foreseen, but one which is to me highly satisfactory, that, in examining the artery, there was no portion from the division into the iliacs down to the femoral artery on which a ligature could be applied, since ossifications had formed in their internal membrane.

I have annexed drawings of the aneurismal tumour, on which Mr. Hunter first successfully operated by taking up the femoral artery, to show how slowly the coagulum is absorbed, as the man lived twenty months after the operation had been performed.

SECT. III.

On the Effect of Heat on Fluid Blood out of the Body.

The heated needle used in coagulating the blood in the aneurismal sac was passed into a drinking horn through the centre of its bottom, and blood from the arm to the amount of three ounces was received into it. The heat was kept up by the lamp half an hour. At the end of twenty-four hours a perpendicular section was made of the coagulum; and that part in contact with the needle, for the extent of half an inch all round, was foliated in the same manner as in the second figure of the representation of the contents of the aneurismal tumour. There was no buff on the upper surface of the coagulum; the mass was uniformly dark red, with minute particles of coagulable lymph, which were white.

To ascertain the effect upon fluid blood produced by different degrees of heat applied to it, three ounces of blood were received into three separate vessels, in each of which was a piece of iron of the same size; one heated by boiling water, one the colour from red heat just gone off, and one red hot. In five minutes, the pieces of iron being removed, the heat conveyed to the blood was ascertained by a thermometer to be 110°, 105°, 85°, the atmosphere 55°. At the end of twenty-four hours a perpendicular section was made of each coagulum.

No. 1. The surface had a greyish film $\frac{1}{10}$ of an inch thick, the coagulum loose in texture, dark red, studded with white, specks. At the bottom was a cell in which the iron had been inclosed: its inner surface resembled that of the aneurismal tumour. Serum seven drachms.

No. 2. A film on the surface $\frac{1}{20}$ of an inch thick, the coagulum more firm, its substance interspersed with white specks, and a cell at bottom as in No. 1. Serum twelve drachms.

No. 3. Much cupped on the surface, the buff $\frac{1}{8}$ of an inch thick, the coagulum solid; no white specks, no cell at bottom.

This experiment was repeated with three balls of lead at lower temperatures, 200°, 160°, 120°, leaving the bullets in the vessels for twenty-four hours.

In that at 200° the surface had a buff $\frac{1}{20}$ of an inch thick, serum four drachms, a buffy coat round the ball $\frac{1}{10}$ of an inch thick.

That at 160° had the surface buffy, $\frac{1}{24}$ part of an inch thick, serum six and three quarter drachms, no film round the ball.

That at 120° much cupped on the surface, buff $\frac{1}{16}$ part of an inch, serum seven drachms, no film round the ball.

Heat of 120° appears to make blood drawn from the arm to have a strong buff.

Blood drawn from the arm into a cup kept at the temperature of boiling water does not form a coagulum, while in a cup kept in ice the coagulum has a buff $\frac{3}{8}$ of an inch thiek; and although there are five drachms of serum, quite loose in texture, these effects of different degrees of

heat on fluid blood in the living body, when out of the direct course of the circulation, as well as those produced on blood drawn from the arm, are new and are very curious facts, which no reasoning, *à priori*, could ever have brought to light, and lead us to a new mode of treating aneurisms.

SECT. IV.

The Blood not decomposed by having been frozen.

Half a century ago, when I began my professional education under Mr. Hunter, he was deeply engaged in investigating the properties of the blood, and ascertaining the changes it underwent under different circumstances. His object in this inquiry was to prove, that the blood possessed within itself a principle of life by which all these changes are regulated.

Under his direction I made the following experiment, which proved that when frozen and thawed it had undergone no change.

Two inches in length of the jugular vein of a sheep, detached while turgid with blood, and the two ends secured by ligatures, were immersed in a cooling mixture and frozen. After the blood had thawed, it was found in a fluid state, and coagulated on exposure, like recently drawn blood from the arm.

SECT. V.

On the Changes the Blood undergoes when mixed with Urine.

The greater number of cases in physic and surgery are only important and interesting to those engaged in the study of medicine; they are not connected with general science, and, therefore, do not properly come under the consideration of the physiologist.

Practitioners, however, in these professions have, upon many occasions, brought to light facts of importance in the animal economy, which could only be discovered while the human body was labouring under disease.

As every change the blood undergoes must appear an object of importance to those who study the economy of animals, I am induced to believe the present observations on the change produced on it by being mixed with urine will not be considered as wholly undeserving of notice.

I was led to pay attention to this subject from considering the following case, which came under my care.

A gentleman, seventy-one years of age, in the spring of 1795, found in making water the urine had the appearance of blood, and congealed into a solid mass as soon as received into the vessel. This complaint appeared to have arisen from the rupture of a vessel in one of the kidneys, for he had a pain in his loins, but none in the region of the bladder. He seemed to void no water; for the whole

quantity which was expelled at any one time, amounting to about four ounces, formed itself into a coagulum: next day he voided bloody water, which did not coagulate. This continued for three or four days, and then went entirely off.

In the spring of 1796 he had a return of the same complaint. It came on in the evening of the 3d of April: on the 4th it was very violent, and in the afternoon there was a total suppression of urine. A catheter was passed six or seven times; but the oval holes near the end of the instrument were always filled with coagulated blood, and no urine could be drawn off. On the 5th a larger catheter was passed, with small round holes, less likely to have the coagulum entangled in them, but no urine came away. In the evening it was introduced again, having its cavity completely lined with a flexible gum catheter, which was withdrawn as soon as the instrument had reached the fundus of the bladder, and in this way four ounces of a bloody fluid were drawn off, which on exposure coagulated.

On the morning of the 6th, a pint of bloody urine was drawn off: this operation was repeated three times in the twenty-four hours, and the same quantity was brought away each time.

On the 7th, the urine drawn off was less tinged with blood, and when it was allowed to stand, the upper part became tolerably clear. There was little change in the circumstances for six days; but on the 13th, the urine drawn off was of a darker red colour, and in smaller quantity. On the 16th the colour was more of a light brown, and after standing some time a white powder was deposited. The

urine drawn off in the morning, upon the patient getting up, was nearly of the natural appearance; but that brought away in the course of the day had a deeper tinge, and more of the white sediment. It is also to be remarked, that the sediment evidently passed off only with the last part of the urine.

On the 19th the urine was tolerably clear, and the white sediment more completely separated and in greater quantity. In the course of the night, while lying in bed, the patient voided naturally, in many different attempts, four ounces of water, but could not make any when up. The urine now continued clear from any tinge: but no more passed without the catheter being introduced, till the 28th, when he again made some water naturally, but could not completely empty the bladder; on the 29th the quantity which required being drawn off was less; and by the 5th of May he made water as usual, at which time the sediment began to diminish, and gradually disappeared.

From the symptoms which have been stated it appears that part of the blood which passed into the bladder from the kidney had remained there, and formed a coagulum, which coagulum gave a bloody tinge to the urine, and caused an inability to void it without assistance, till the coagulum was dissolved.

With a view to ascertain how far this had been the case, and discover what changes the blood undergoes when placed in such circumstances, I instituted the following experiments. They were performed by Mr. Charles Grover, a very ingenious surgeon, at that time house surgeon in St. George's Hospital.

Experiment I.

Four ounces of blood were drawn from the arm into a phial containing four ounces of urine just voided, and the phial was kept in the temperature of the human body: in fifteen minutes the whole mixture formed an uniform firm coagulum, and appeared wholly composed of blood.

This experiment was made to ascertain the probable time the blood would take to coagulate in the bladder.

Experiment II.

Six ounces of blood were drawn from the arm into six ounces of recently made urine: in fifteen minutes the whole mass became one solid coagulum. In seven hours six drachms of a clear fluid were separated from it. This was poured off, and the same quantity of urine added; and after nine hours it was poured off, some red globules were mixed with it, but sunk to the bottom undissolved. The coagulum had fresh urine added to it three times a day, the former urine being previously poured off, and allowed to stand some hours for examination.

For the first five days the coagulum appeared to undergo little change, except becoming smaller in size, and the urine

poured off from it was tolerably clear, but on standing deposited a dark cloudy sediment.

On the sixth day the urine poured off from the coagulum was of a dark red colour, and deposited a greater quantity of a dark-coloured sediment, but on standing became tolerably clear.

On the ninth day the coagulum was reduced to the size of the original quantity of blood drawn from the arm.

On the thirteenth day the size of the coagulum was a good deal reduced; the urine passed off from it was still more tinged with the red globules; but when allowed to stand, the upper part became clear, free from the red tinge, and the sediment had the appearance of a whitish powder.

From this time the quantity of white sediment increased, and the size of the coagulum diminished. In its decrease the loss was from its external surface, and nearly equally all round; what remained appearing like the nucleus of the original coagulum. On the twenty-fifth day it was of the size of a large cherry, and on the twenty-ninth it entirely disappeared. Some red globules were very distinctly seen in the sediment along with the white powder.

To see how far the changes the blood had undergone in this experiment depended on the peculiar properties of the urine, the following experiment was made with blood and common water.

Experiment III.

Six ounces of blood were drawn from the arm into six ounces of water. In a quarter of an hour the whole became one solid coagulum. In twelve hours six ounces of a clear water, of a bright red colour, were separated, nor did it on standing deposit any sediment.

This coagulum had fresh water added to it twice a day, and what was poured off was allowed to stand for examination.

The coagulum on the second day began to break; on the fifth had a putrid smell; and in eighteen days was almost entirely dissolved.

The water which was poured off was of a bright red colour from the beginning to the end of the experiment, in consequence of the red globules being dissolved: it had a very offensive smell, but never deposited any white sediment, the coagulable lymph dissolving from putrefaction.

As it is evident, from the result of the last experiment, that the coagulum remaining so long undissolved in the second experiment, depended upon its being mixed with the urine, I was desirous of knowing whether it was the urine incorporated with the coagulum, or that which surrounded it, which produced this effect. To determine this point I instituted the following experiment.

Experiment IV.

Four ounces of blood were drawn from the arm into a cup, and allowed to coagulate. Four ounces more were drawn into a separate cup. From each of these equal portions of coagulum, at the end of three hours, one ounce of serum was separated, and poured off. To one of them fresh urine was added, to the other common water. The urine and water were changed night and morning.

The water was tinged of a bright red colour throughout the whole experiment, and deposited no sediment. On the eighth day the coagulum was rather looser in its texture. On the thirteenth day it began to break, and by the twentieth day it was nearly dissolved; the progress corresponding with that of the coagulum in Experiment III.

The urine the second day of the experiment was clear, but the bottom of the basin was covered with red globules undissolved.

On the fifth day the urine poured off was tinged of a bright red colour, similar to the water taken from the other coagulum, and after standing some hours a white sediment was deposited. On the thirteenth day it was looser in texture, and more dissolved than the coagulum in the water. It continued to tinge the urine of a bright red colour, and what was poured off deposited a white sediment in greater quantity. On the eighteenth day the coagulum was nearly dissolved, so that the coagulum immersed in the urine dissolved two days sooner than that in the water.

From this experiment we find that it was the urine incorporated with the coagulum in Experiment II. that prevented the red globules from dissolving, and preserved the coagulum for so long a time, since these effects were not produced by urine while simply surrounding the coagulum.

If we compare Experiment II. with the result of the case that has been detailed, they agree so entirely, that it leaves no doubt of the process carried on in the bladder being similar to that which took place out of the body. The patient was unable to make water for twenty-four days, although the passages readily admitted during the whole of that time an uncommonly large instrument, which could not have been the case had there been any obstruction in them: for six days more he voided it with difficulty, but afterwards made water very well.

The coagulum out of the body was reduced in twenty-five days to the size of a cherry, and in four days more it was completely dissolved.

The patient's urine became darker from the red globules mixing with it in nine days. In the experiment this took place in five days.

The white sediment was first observed, in both instances, about the twelfth day; it continued to be deposited till the patient got well, and to the end of the experiment.

That the blood is capable of uniting with a quantity of urine equal to itself so as to form a firm coagulum; that the red globules do not dissolve in a coagulum so formed; that an admixture of urine prevents the blood from becoming putrid, and that the coagulable lymph breaks

down into parts resembling a fine powder, are facts which I believe to be new; they are certainly not generally known. Considered abstractedly, they may not appear of much importance, but when compared with what takes place in the urinary bladder in the living body, and found to agree with the process the blood undergoes when deposited there, they become of no small value, since they enable us to account for the symptoms that occur in that disease, and lead us to the most simple and effectual mode of relieving them.

SECT. VI.

On Respiration.

No subject connected with physiological enquiry has more excited the attention of the anatomist and chemist than Respiration; but as this investigation has, till very lately, always been considered to involve (some way or other) the production of animal heat, till that error was corrected little progress could be made in our enquiries into this very important and intricate subject.

The production of animal heat being decidedly one of the functions of the ganglions and nerves, as we shall prove in the present volume, and in no respect connected with the circulation of the blood, whether through the lungs or the body, brings respiration before us in a more simple form, and removes many embarrassments we had before to contend with in this enquiry.

Respiration, as well as animal heat, are even now considered as belonging to the science of chemistry; and I may probably be thought in taking it up to have gone beyond my province, as well as my depth. I, on the other hand, am inclined to believe that the process of respiration is more simple in itself than is imagined, and more within the reach of discovery by means of an accurate knowledge of the parts employed, than by having acquired an intimate acquaintance with the intricacies belonging to chemical affinities.

It is a subject that certainly admits of being investigated chemically, independent almost of anatomical acquirements; and in this way only has it hitherto been examined. But I must contend that such investigation is, in many respects, imperfect; and however plausible the conclusions that are obtained, we must not consider them established, unless the mechanism of the lungs and the mode in which the blood circulates through those organs in all respects correspond to the chemical processes that are stated to be carried on in them.

The chemists lay down the following facts, which are considered to be completely established upon the firm basis of experiment: —

1. That atmospheric air consists generally of one part oxygen, and fonr parts by measure of nitrogen.

2. That whatever quantity of atmospheric air is inhaled at one inspiration, the same bulk exactly is exhaled in the following expiration; but though the quantity is the same, a part of the oxygen disappears, and is replaced by the same bulk of carbonic acid gas.

3. That carbonic acid gas, when examined by weight, is made up of six parts carbon, and sixteen oxygen. When examined by measure, one cubic inch of oxygen enters into the composition of one cubic inch of carbonic acid gas.

From the exact coincidence in bulk between the expired and the inspired gas, and the curious circumstance that carbonic gas united with oxygen produces no increase in bulk, the chemists explain that in respiration there is no loss of oxygen, but that the expired gas contains all that was inspired, and also as much carbon as was required to

form the oxygen into carbonic acid gas; by which means the blood, having lost so much carbon, is purified, and when it returns to the heart is fitted for carrying on the functions of life.

Nothing could be more ingenious than this theory, were it proved that the only purification the blood requires in respiration is the removal of a portion of carbon from the blood in the lungs. But as carbonic acid gas is detected in the urine as well as in the matter perspired, it is evident that oxygen in a pure state is necessary for the functions of arterial blood, and only one of its uses is uniting with any superfluous carbon, and carrying it out of the arterial system by these secretions. We have, then, three modes of decarbonizing the blood by means of oxygen: two peculiar to the arterial blood, and one confined to the blood in the pulmonary artery by the air-cells of the lungs; but no mode is pointed out by which the heart and arteries are supplied with oxygen to replace that which has been consumed; and neither M. Guy Lussac in Paris, Professor Brande, nor Mr. Farrady, could satisfy my doubts on this subject.

Finding, from the candid acknowledgment of these three able chemists, that they had not fully made up their minds respecting the validity of the commonly received opinion, also being told by them that carbon has never been met with in living animal bodies in the state of carbon, but always chemically combined with other substances, and that every process by which it can be disengaged with which they are acquainted is attended with the production of heat, I think it fair to consider the circumstance of heat not being evolved in respiration as a strong objection to

this theory; and, therefore, I set to work to examine the question anatomically as well as chemically. In this I have had the assistance of Mr. Russell and Mr. Bauer for acquiring a more correct knowledge of the structure of the lungs.

To determine the effect produced upon the pulmonary circulation by respiration, I got Mr. Russell to inject the arteries of the lungs of a sheep, and found that the minute injection commonly used by anatomists very readily returned by the veins; but when the cells were previously distended, by whatever means, this did not take place.

From this fact, which is not founded on a single experiment, we learn a very important truth; that while the cells are full, the terminal branches of the pulmonary artery are prevented from sending the blood on to the veins, but the blood in the veins meets no obstruction in passing to the heart.

To determine whether the terminal arteries, that supply the coats of the cells with blood, correspond in their mode of ramifying, with the originating venal branches that receive and convey it to the heart, I put the injected lungs into Mr. Bauer's hands for a more minute examination than I could give them.

From the delicacy of the texture of the air-cells themselves, and the numerous branches of vessels of different kinds by which they are surrounded, many difficulties were to be overcome before all the parts could be brought distinctly before the eye: this was at last effected by injecting the air-cells with quicksilver, the weight and fluidity

of which distended them equally. The branches of the pulmonary veins were filled with yellow, and the arterial branches with red injection: the portion of lung was then plunged into rectified spirit of wine.

From preparations of this kind, varied in different ways, which it is not necessary to particularize, Mr. Bauer has opened a field of minute anatomy never before explored, and without a knowledge of which the most important functions of animal life could never have come within the reach of being explained.

In the highly magnified microscopical drawings annexed, we have the exact size of the cells of the lungs, their configuration, and, by magnifying them in different degrees and exposing sections of them, the vessels that ramify upon their internal membrane are distinctly seen, and their structure, as well as course, clearly defined. We have, in fact, a camera obscura opened to the eye, in which are seen upon the opposite side of the cell, and projecting from behind the internal lining, arteries running in the directions they usually follow, veins much larger in proportion than the arteries anastomosing with one another, and having valves to prevent the regurgitation of their contents. Absorbents turgid with gas passing from the cells to the veins, and both these vessels being furnished with valves, would rapidly convey the gas to the left auricle of the heart, in which the veins terminate. The parts represented are so distinctly seen and correspond so exactly in their appearance with branches of similar vessels in the other parts of the body, that no more particular description of them appears to be called for; and as this is, in fact, the only

representation of a section of a cell of the human lungs that has ever been so much magnified, we have nothing with which to compare it. As none of my chemical friends could inform me, whether arterial blood contained a sufficient proportion of carbonic acid gas to form similar tubes in the act of its evolvement, as in the coagula of venal blood, nor acquaint me with the proportion of oxygen usually met with in the arteries, I thought both subjects were deserving of consideration; and as a portion of the carbonic acid gas formed in the arteries is carried off by the secretions, so the carbonic acid gas formed in the veins may be carried off by means of the cells of the lungs, without any elaborate chemical process of separating carbon from any other compound to have it recombined with oxygen.

Having caught this new view of so very important and interesting a subject, I instituted the following experiments on the blood: they were made in the laboratory of the Royal Institution at noon on the 3d of June, 1826.

Four ounces of blood taken on the preceding evening from the carotid artery of a sheep into a glass vessel, and the same quantity from the jugular vein into another glass vessel, were made the subjects of a comparative experiment.

The quantity of serum separated was nearly the same in both vessels.

The arterial blood had the film on the surface of a bright red colour, which was seen through the glass to be continued some way down the sides of the coagulum, by which it was readily distinguished from the venal coagulum.

These two vessels, prepared as in former experiments, and

both of them in exactly the same manner, were placed under the receiver of an air-pump, with fluid minute injection in each; and the changes the blood underwent while the exhaustion was going on were observed.

From the venal blood large globules of gas were evolved. When these arrived at the surface, they forced their way out and disappeared, leaving cavitiesas large as peas.

From the arterial coagulum small globules like seed pearls formed a row round the edge of the glass upon the surface of the injection: they were all of the same size.

On the 5th the two coagula had their surfaces washed in distilled water, and were immersed in proof spirit. When examined by Mr. Bauer, the venal coagulum was found to be injected; the arterial had received no injection into its substance.

To ascertain chemically the difference in the proportions of oxygen and carbonic acid gas in arterial and venal blood, four ounces of blood from the carotid artery, and the same quantity, or nearly so, from the jugular vein of the same sheep, were deposited in vessels. The vessels were immediately closely covered with bladder, and the gas collected in twenty-four hours, under the bladders, was examined.

The arterial blood contained four and a quarter dram measures of gas. Of this not more than $\frac{1}{25}$th part was carbonic acid gas, the remainder nitrogen, with a proportion of oxygen sufficient to support flame, but less than is contained in the atmosphere.

The venal contained two and a half dram measures of gas. Of this $\frac{1}{16}$th part was carbonic acid gas, the remainder nitrogen with a little oxygen, which extinguished flame.

The arterial coagulum was bright in its colour, the venal dark.

With a view to observe the effects on the blood produced by overcharging it with oxygen, compared with those that result from overcharging it with carbonic acid gas, the following experiment was made on May 17. 1826: —

Blood from the arm, to the amount of five ounces, was received into a bottle, whose capacity was twenty-four ounces, filled with oxygen, and the stopple, which was taken out to admit the blood, was immediately replaced.

The same quantity from the same arm was received into a similar bottle filled with carbonic acid gas.

In twenty-four hours these two bottles, which had been kept in a cool place closely stopped, had their coagula taken out and examined. The serum separated in both was the same in quantity: the appearance of the coagula did not differ.

They were then submitted to the air-pump with fluid injection, an equal portion from both having been removed.

The oxygenated coagulum parted with small globules, similar to those from arterial blood in the former experiments, which when they arrived at the surface disappeared.

The carbonated coagulum was longer before any globules were evolved, and when they appeared they were more than double the size of those in the other vessel; and numerous large bubbles burst forth from the cut surface.

When the exhaustion was completed, the air was let in; and in forty-eight hours from the commencement of the experiment the oxygenated coagulum retained its florid colour, which the other had lost.

In seventy-two hours the florid coagulum had no injection in any part of its substance, and, therefore, had lost its carbonic acid gas. Immediately under the film by which its surface was covered an infinite number of small cells were formed.

At the same period the dark-coloured coagulum had tubes formed in it filled with injection.

Both coagula were kept for four days longer, and during the whole of that time small globules were seen to rise from the florid coagulum, while none were evolved from the other. From this circumstance I am induced to believe that a large proportion of oxygen is absorbed by the blood, and is retained a long time after coagulation takes place; and when evolved it forms globules like other gases, but not tubes, as carbonic acid gas does. How far physiologists are agreed upon the subject of the renewal of the heart's action depending upon oxygen being applied through the medium of the blood to the muscles of that organ after its action has ceased, I will not stop to consider; but as such renewal of action is allowed to depend upon respiration being re-established, any observations on the mode in which this can affect the heart's action are important to the present enquiry.

It is curious that although respiration is necessary for carrying on the functions of life, it is by no means so for the continuance of its existence. The garden snail illustrates this fact in the most satisfactory manner: when the temperature of the atmosphere sinks below a certain degree, this animal places itself upon a solid body, that it may not be liable to fall off; it then forms an operculum of mucus, by which respiration is stopped, and the animal

remains hermetically sealed up till warmth and moisture dissolve the mucus by which the animal was fixed to its place, and a globule of air retained in the lungs, which consist of only one cell or bag, being rarefied, escapes externally, restoring the communication with the air of the atmosphere which rushes in, and the action of the heart is renewed. If it is admitted that the application of oxygen to the muscles of the heart is capable of stimulating that organ, nothing can be more simple than the mode in which this is effected: the oxygen of the atmosphere is absorbed by the blood in the lungs, and the closeness of the ventricle of the heart to the lungs permits the oxygen to penetrate to the heart.

When a quadruped has had its head cut off, and the heart has ceased to act, the action is restored by means of artificial breathing, and has been kept up for twenty-five minutes in an experiment of Mr. Brodie's, recorded in the Phil. Trans. The animal losing its heat during this process sufficiently proves that animal heat is unconnected with respiration.

In what manner can the process of decarbonising the blood by respiration restore the heart's action, since the circulation of the blood cannot be renewed till that action has taken place? If this is only to be done by carrying oxygen to the blood contained in the left auricle, how is it to be effected by this process? The blood is every where at rest, the brain is removed, and the air-cells are distended with atmospherical air by the bellows: the oxygen in the cells decarbonises the blood in the pulmonary arteries by attracting its carbon; and the second action of the bellows

empties the cells, and the carbonic acid gas is expelled. The cells are filled a second time by the bellows, more carbon is attracted by the fresh dose of oxygen, and then by the fourth action of the bellows expelled. This decarbonisation applies to the blood loaded with carbon at the time the circulation ceased; but the blood which had its carbon removed by the last expiration that took place previous to the heart's motion being stopped, may be said to have been in a pure state, and had no carbon to give out. In what way, then, can this decarbonisation of the blood create and convey a new and very powerful stimulus to the muscles and nerves of the left auricle of the heart, by which its action is renewed?

Unless this can be satisfactorily explained, the present theory must be abandoned, more especially if it can be shown that such a stimulus can, in another way, be produced, and directly applied to the auricle, and capable of exciting its action.

Now that the cells of the lungs and the different vessels with which they are furnished have been so beautifully displayed in Mr. Bauer's drawings, it is evident that, in the process of respiration, oxygen must be absorbed at the same time that carbonic acid gas is discharged. The size and number of the absorbents, and their frequent valves, will afford facilities for the oxygen inhaled being absorbed by the blood in the pulmonary veins, and in that way reach the heart, more particularly as at that time the communication between the arteries and veins is stopped; and the oxygen is readily detached from the nitrogen, since Professor Brande doubts whether they are otherwise united than by

being mechanically mixed together. The supply of carbonic acid gas furnished by digestion, as explained in a former part of this work, will always enable the branches of the pulmonary artery to replenish the cells of the lungs, measure for measure with carbonic acid gas, for the oxygen carried off leaving still a portion in the venal blood.

In the year 1816, I was present when Mr. Brodie killed an ass by the woorara poison. After the animal was dead, and allowed to be past recovery, artificial breathing restored it to life. The mode in which this was effected I presume to be,—a supply of oxygen was carried from the cells of the lungs, and when it reached the heart, by its stimulus renewed the action of that organ.

This view of respiration accords with the observations made on breathing vital air, published by Mr. Hill.

His patients breathed it mixed with common air in three different proportions; in that of one measure to forty, one to twenty, and one to ten; the alterative course of it as a medicine was one to twenty once a day, or three times a week. Its immediate effect was producing redness on the skin, more especially the cheeks and lips: it afterwards increased the action of the heart and arteries; it also promoted urine. An improvement in the patient's strength, and an increase of growth, was evident in a week or fortnight.

The wonderful effect of oxygen upon the growth of plants, beyond that of atmospheric air, is shown by an engraving which makes the frontispiece.

Mr. Bauer informs me, that Mr. Otto, superintendent of the Royal Botanical Garden at Berlin, took seeds from a

hortus siccus, that had remained there more than a century, and after putting them a few days in oxygen, sowed them; they grew and did well. This was done by the advice of a German chemist, who had employed himself in making experiments with oxygen.

SECT. VII.

On the Effect produced upon the Air-Cells of the Lungs when the Pulmonary Circulation is too much increased.

On comparing the structure of the lungs in the quadruped and cold-blooded animals with that of the human species, it was found that in the hare and sheep the branches of the bronchiæ had cartilaginous rings continued almost as far as the cells in which they terminate; and in the turtle there are no perfect air-cells, but the whole substance into which the bronchiæ open is a species of trellis-work.

In examining the lungs of a hare that had been coursed by greyhounds, it was found that the superficial air-cells, which both in the human lungs and those of the hare are larger than the more internal ones, were filled with colourless coagulable lymph, in a solid form, which appeared like white specks, and the smaller more internal cells were loaded with extravasated blood; but no such appearance was met with when the hare had been snared or shot; so that it was natural to conclude, that it must be produced by the over-exertion of the animal's speed in endeavouring to escape from the dogs.

That I might better ascertain this point, I procured two hares, killed after an unusual long run, and found in both of them the appearance just described: it was more general in one hare than the other. This probably arose from the animal having made the greatest exertion.

The appearance is shown in the drawing, which also gives the exact form of these superficial air-cells.

Never having been out coursing, I applied to the game-keeper of Richmond Park, where hares are coursed regularly during the season. Mr. Sawyer informed me that a run of fifteen minutes with greyhounds was rarely exceeded; and when the animal has been pressed for so great a length of time, it often sinks from exhaustion, and dies before the greyhounds reach it. The dogs themselves upon these occasions are so blown, as often to be unable to seize their prey, although they have come up with it. A run of fifteen minutes with greyhounds is considered to exhaust the hare equally with a run of three hours with harriers.

That the air-cells in a coursed hare might be more accurately examined by Mr. Bauer, the bronchiæ were filled with mercury, part of which was received into the cells containing the lymph; the parts were then plunged into rectified spirit, that they might harden and not be allowed to collapse. Some days after, the parts were examined in the microscope, and it was found that the air-cells not only had received part of the mercury, but the rest had been retained in the branches of the bronchiæ, which was not found to be the case when the air-cells of the human lungs were filled with mercury, the elasticity of the membranous tubes by which the bronchiæ open into the cells having squeezed out the quicksilver.

This difference of structure in the terminal branches of the bronchiæ in the hare will give a more ready passage to the air so as to facilitate its respiration, while the membranous terminations of the human bronchiæ admit of the

volume of air in the lungs being varied according to circumstances, as we find is the case in singing and playing on wind instruments, which I understand is often attended with bad consequences in those individuals whose lungs are delicate in their texture, and if persevered in has proved fatal.

The coagulable lymph forced into the cells must have been separated from the blood in its passage through the smaller branches of the pulmonary artery, in the same manner as in the inflammation of veins it is deposited on the internal membrane, while the blood is continuing its course along the vein. Many instances of such separation, where heat is applied to the circulating blood, have been brought forward in this volume.

Every thing that is valuable in the pursuits of Comparative Anatomy arises from their making us better acquainted with the structure of the human body, and the uses of its different organs, which is in no other way to be acquired. In what way can our knowledge in human anatomy and physiology be improved, so as to enable us to learn the rudiments of the healing art for the relief of the sufferings to which humanity is liable, but by consulting and examining the other works of the Almighty, who is the Author of our being, and has given his creatures for our use, not more for the purposes of affording food and raiment, than to make us better acquainted with the mechanism of our own bodies?

I have been led to this remark, upon the present occasion, from a belief that the specks of coagulable lymph which distend the superficial air-cells of the lungs, whenever the

circulation through them is hurried beyond what the structure of the organ can bear, is an effect similar to the formation of tubercles in the human lungs, one of the most general, and, I may say, destructive diseases, to which women and children in this country are liable.

Of the nature of tubercles in the human lungs no correct notion has been formed, and the effect produced in the hare by an over-exertion of its speed, is the only thing I have met with which throws any light on their formation. In proof of this I need only compare it with the account given by Dr. Baillie, in his very valuable work on Morbid Anatomy, and the representations that are referred to in the plates which are annexed. No one had a more sound judgment, or was less liable to be led away from the facts that came within the reach of his observation; and no one could have more ample opportunity of noting the symptoms in the living body, or of examining the appearances after death.

His words are:—"Tubercles consist of rounded bodies of a white colour, interspersed through the substance of the lungs. They are, probably, formed in the cellular structure which connects the air-cells of the lungs together, and are not a morbid affection of glands, as has been frequently imagined. There is no glandular structure in the cellular connecting medium of the lungs. On the inside of the bronchiæ, continued from the trachea, where there are folicules, tubercles have never been seen. They are, at first, very small, not being larger than the heads of very small pins, and in that case are frequently accumulated in small clusters. The smaller tubercles of a cluster grow, probably,

together, and form one large one. The more ordinary size of a tubercle is about that of a garden pea, but they are subject in that respect to great variety. They adhere closely to the substance of the lungs, but have no particular covering or capsule, and have little or no vascularity. When cut into, they are found to consist of a white, smooth substance, having a firm texture, and often contain in part a thick curdly pus."

Dr. Baillie has given two plates, in each of which there are two figures. In the first the tubercles are of a small size; but from that representation no conclusion can be drawn either as to their origin or their real situation respecting the healthy structure of the organ. In the second, which he mentions to be more rarely met with, they are shown both on the convex and concave surfaces, close to one another, immediately under the surface, and projecting through the pleura covering the lung. They have acquired a considerable size; and it is much to be regretted that they were not met with in an earlier stage, and that the history of this case has not been registered: but when the appearance represented and the section of the tubercle are accurately examined, no one can doubt that the origin of this species of tubercle must have been from particles of coagulable lymph deposited in the superficial air-cells, similar to those met with in the hare.

SECT. VIII.

On the Poison of the Rattlesnake.

Opportunities of tracing the symptoms produced by the bite of poisonous snakes, and ascertaining the local effects on the human body when the bite proves fatal, are of such rare occurrence, that no well described case of this kind is to be met with in any of the records that I have examined. I am, therefore, induced to give in detail the following account, with the view of elucidating this subject, in which the interests of humanity are so deeply concerned: —

Thomas Soper, twenty-six years of age, of a spare habit, on the 17th of October, 1809, went into the room in which two healthy rattlesnakes, brought from America in the preceding summer, were exhibited. He teased one of them with the end of a foot-rule, but could not induce the snake to bite it, and on the rule dropping out of his hand, he opened the door of the cage to take it out; the snake immediately darted at the hand, and bit it twice in succession; making two wounds in the back part of the first phalanx of the thumb, and two on the side of the second joint of the fore-finger. The snake is between four and five feet long, and when much irritated bites the object twice, which I believe snakes do not usually do.

The bite took place at half past two o'clock. He went immediately to Mr Hanbury, a chemist in the neighbour-

hood. There was at that time no swelling on the hand, and the man was so incoherent in his language and behaviour, that Mr. Hanbury considered him to be in a state of intoxication, and gave him a dose of jalap to take off the effects of the liquor, and made some slight application to the bites. It appeared, on enquiry, that the man had been drinking, but that before he was bitten there was nothing unusual in his behaviour. After leaving Mr. Hanbury, the hand began to swell, which alarmed him, and he went to St. George's Hospital. He arrived there at three o'clock. The wristband of his shirt had been unloosed, and the swelling had extended half way up the fore-arm before his admission. The skin on the back of his hand was very tense, and the part very painful. At four o'clock, the swelling extended to the elbow, and at half past four it had reached half way up the arm, and the pain had extended to the axilla. At this time Mr. Brodie, who visited him in my absence, first saw him: he found the skin cold, the man's answers were incoherent, his pulse beat 100 strokes in a minute, and he complained of sickness. Forty drops of aqua ammoniæ puræ, and thirty drops of spiritus ætheris vitriolici in an ounce of mistura camphorata, were given to him, but did not remain on his stomach. The wounds were bathed with the aqua ammoniæ puræ, and the arm and fore-arm had compresses wetted with camphorated spirit applied to them. At five o'clock he took two drams of spiritus ammoniæ compositæ, and thirty drops of æther in an ounce and a half of mistura camphorata, which remained on his stomach. At six o'clock his pulse was stronger; at half past seven his pulse was very

feeble, and thirty drops of æther, with the same quantity of aqua ammoniæ puræ were given in water. At half past eight it was repeated. At nine o'clock he had the feeling of great depression, his skin was cold, his pulse weak, beating eighty strokes in a minute. The dose was increased to fifty drops of both medicines, and repeated. At a quarter past ten o'clock the pain had become very violent in the arm, his pulse was stronger, but fits of faintness attacked him every fifteen minutes, in which the pulse was not perceptible, but in the interval his spirits were less depressed. In the course of the evening he had two stools. At half past eleven o'clock I first saw him. The hand, wrist, fore-arm, and arm, were much swelled up to the shoulder, and into the axilla. The arm was quite cold, and no pulse could be felt in any part, not even in the axilla, the swelling preventing me from feeling the axillary artery with any degree of accuracy. The wounds made on the thumb were just perceptible, those on the finger were very distinct. His skin generally was unusually cold. I took some pains to diminish his alarm of danger, and found his mind perfectly collected: he said he hoped he should recover. At one o'clock in the morning of the 18th, he talked indistinctly, his pulse beat 100 in a minute: the attacks of faintness came on occasionally. The medicine was repeated every hour.

At eight o'clock in the morning of the 18th, his pulse beat 132 strokes in a minute, and was very feeble. The swelling had not extended beyond his shoulder to the neck, but there was a fulness down the side, and blood was extravasated under the skin as low as the loins, giving the

back on the right side a morbid appearance. The whole arm and hand was cold, but painful when pressed: the skin was very tense. On the inside of the arm, below the axilla, and near the elbow, vesications had formed; and under each of the vesications there was a red spot on the cutis, of the size of a crown piece. The skin generally over the body had become warm. He was low and depressed: there was a tremulous motion of his lips; and the faintings recurred at nearly the same intervals as in the preceding evening. The last dose of medicine was rejected by vomiting, but some warm wine remained on his stomach. The arm was fomented.

At twelve o'clock, in addition to the above symptoms, there was a starting of his limbs. He had attempted to take some broth, but his stomach did not retain it. The skin of the whole arm had a livid appearance, similar to what is met with in a dead body when putrefaction has begun to take place, unlike any thing which I had ever seen in so large a portion of the living body. An obscure fluctuation was felt under the skin of the outside of the wrist and fore-arm, which induced me to make a puncture with a lancet, but only a small portion of a serous fluid was discharged. My colleague, Dr. Nevinson, was present at this visit, and we agreed to continue the internal use of the volatile alkali, with the view of rousing the stomach to action, not considering it as having any specific power over the poison. At eleven o'clock in the evening, finding that his stomach did not always retain the medicines, nor even small quantities of brandy which were given him, I directed the volatile alkali to be left off, and two grains of opium to be

given, and repeated every four hours. At this time his pulse was scarcely perceptible at the wrist; the fainting fits were not less frequent. The vesications and red spots were increased in size.

October 19. At nine o'clock in the morning his pulse was scarcely perceptible, his extremities were cold, the vesications were larger, and the size of the arm was diminished. He was drowsy, probably from the effect of the opium. He had taken nothing but brandy during the night. At three o'clock in the afternoon he was more depressed; spoke only in whispers; the vesications were increased; the fainting fits less frequent. The arm was diminished in size, and he had sensation in it down to the fingers. At eleven o'clock at night his pulse beat 130 in a minute, and was low. The opium was left off. A stool was procured by clyster. He was ordered to have a glass of camphorated mixture occasionally, and wine and brandy as often as he could be induced to take them.

October 20. He had dozed at intervals during the night; his spirits were better, and his extremities warmer. At nine o'clock he took coffee for breakfast.

He afterwards took some fish for dinner, but it did not remain on his stomach; he therefore took brandy and coffee at intervals, half an ounce at a time, as larger quantities did not remain on his stomach.

October 21. He had slept at intervals during the night, but was occasionally delirious; his pulse 120 in a minute. Brandy and jelly were the only things that staid on his stomach. The size of the arm was reduced, but the skin was extremely tender.

October 22. He had slept during the greatest part of the night; his pulse beat ninety-eight in a minute: he took some veal for dinner, and brandy at intervals. In the evening his pulse became full and strong: he was ordered wine instead of brandy. The right side of the back down to the loins was inflamed and painful, and had a very mottled appearance from the extravasated blood under the skin.

October 23. His pulse continued full, and the arm was very painful, though reduced in size. The vesications had burst, and the exposed cutis was dressed with white ointment. Stools were procured by an opening medicine. He took some veal and porter for dinner; the wine was left off. In the evening he had a saline draught with antimonial wine.

October 24. There was no material change.

October 25. His pulse had increased in frequency, but in other respects he was nearly the same. His bowels were opened by medicine.

October 26. The arm was more swelled and inflamed.

October 27. The inflammation of the arm had increased: his tongue was furred, and his pulse was very frequent. He attempted to sit up, but the weight of the arm and the pain prevented him. The arm was bathed with spirit of wine and aqua ammoniæ acetatæ in equal quantities.

October 28. A slough had begun to separate from the inside of the arm below the axilla, and a purging had come on, for which he was ordered chalk-mixture and laudanum. In the night he had a rigour.

October 29. The purging had abated: his pulse beat 100 in a minute, and was feeble. A large abscess had formed on the outside of the elbow, which was opened, and half a pint of reddish-brown matter was discharged, with sloughs of cellular membrane floating in it. The lower part of the arm became much smaller, but the upper part continued tense. A poultice was applied to the wound. The lower portion of the arm and the fore-arm were covered with circular strips of soap cerate. He was ordered to take the bark, and allowed wine and porter.

October 30. The redness and swelling of the upper part of the arm had subsided; the pulse was 100 in a minute. The purging had returned. The bark was left off: the chalk mixture and laudanum were given, and an opiate clyster administered.

October 31. The pulse beat 120 in a minute. The discharge from the abscess had diminished; the purging continued, and at night he had a rigour.

November 1. The pulse was 120. His voice was feeble: he had no appetite: was delirious at intervals. Ulceration had taken place on the orifice of the abscess, so that it was much increased in size. He drank two pints of porter in the course of the day.

November 2. His pulse was very weak: his countenance was depressed: his tongue brown: the ulceration had spread to the extent of two or three inches. Mortification had taken place in the skin nearer the axilla. His stomach rejected every thing but porter: in the night he was delirious.

November 3. The mortification had spread considerably;

the purging continued: the forefinger, which had mortified, was removed at the second joint.

November 4. He died at half past four o'clock in the afternoon.

Sixteen hours after death the body was examined by Mr. Brodie and myself, in the presence of Mr. Maynard, the house surgeon, and several of the pupils of the Hospital.

With the exception of the right arm, which had been bitten, the body had the natural appearance. The skin was clear and white, and the muscles contracted.

The wounds made by the fangs at the base of the thumb were healed; but the puncture made by the lancet at the back of the wrist was still open. That part of the back of the hand which immediately surrounded the wounds made by the fangs, for the extent of an inch and a half in every direction, as also the whole of the palm, was in a natural state, except that there was a small quantity of extravasated blood in the cellular membrane. The orifice of the abscess was enlarged, so as to form a sore on the outside of the arm, elbow, and forearm, near six inches in length. Around this the skin was in a state of mortification, more than half way up the outside of the arm, and as far downwards on the outside of the forearm. The skin still adhered to the biceps flexor muscle in the arm, and flexor muscles in the forearm, by a dark-coloured cellular membrane. Every where else in the arm and forearm, from the axilla downwards, the skin was separated from the muscles, and between these parts there was a dark-coloured fluid, with an offensive smell, and sloughs of cellular membrane, resembling wet tow, floating in it.

The muscles had their natural appearance every where, except on the surface which was next the abscess. Beyond the limits of the abscess blood was extravasated in the cellular membrane, and the appearance was observable on the right side of the back as far as the loins, and on the right side of the chest over the serratus major anticus muscle.

In the thorax the lungs had their natural appearance. The exterior part of the loose fold of the pericardium, where it is exposed, on elevating the sternum was dry, resembling a dried bladder. The cavity of the pericardium contained half an ounce of serous fluid, which had a frothy appearance, from an admixture of bubbles of air. On cutting into the aorta, a small quantity of blood escaped, which had a similar appearance. The cavities of the heart contained coagulated blood.

In the abdomen, the cardiac portion of the stomach was moderately distended with fluid; the pyloric portion was much contracted; the internal membrane had its vessels very turgid with blood. The intestines and liver had a healthy appearance. The gall-bladder was moderately full of healthy bile. The lacteals and the thoracic duct were empty; they had a natural appearance.

In the cranium, the vessels of the pia mater and brain were turgid with blood; the ventricles contained rather more water than is usual, and water was effused into the cells connecting the pia mater and tunica arachnoides. It is to be observed, that these appearances in the brain and its membranes are very frequently found in cases of acute diseases which terminate fatally.

The following cases were sent from India to my late friend Dr. Patrick Russel: they arrived after his death, and Mr. Claude Russel very kindly gave them to me, knowing the subject of them to be one in which I had taken an interest.

As they correspond in many circumstances with that which has been detailed, I have inserted them in this place, as well as an experiment which I had an opportunity of making in the West Indies, on the effects of the snake's poison on animals.

A boy, a slave of a gentleman in India, was bitten by a snake, called kamnlee by the natives, in the lower part of the arm, at eight o'clock in the evening. The blood flowed very freely for some time. He died next day at noon in great pain.

A sepoy, sixty years of age, was admitted into the hospital of his regiment, under the care of Mr. Perrin, assistant-surgeon, at four o'clock in the afternoon of the 15th of October 1802, in consequence of his being bitten by a cobra di capello on the back part of the hand. At the time of his admission he complained of pain running up the arm. He immediately took a drachm of eau de luce, and this dose was repeated every half hour, and the same remedy was applied externally as a lotion to the arm and forearm.

At four o'clock in the morning of the 16th of October, the pain began to increase, and the arm to swell with great hardness and stiffness, and tumour in the axilla, with much inclination to vomit. He took twelve grains of Dr. James's powder, which brought up a great quantity of bilious matter.

He drank copiously of warm water, but no perspiration was induced. He appeared relieved for a short time. At eight o'clock in the morning the arm was distended, painful, and discoloured. He took four ounces of brandy, and repeated it every hour until twelve o'clock, with a drachm of eau de luce occasionally. At this time he was a little revived. The brandy was reduced to two ounces, which were carefully and regularly given every hour, until twelve at noon on the 17th of October. The arm was more free from pain, but much swelled, hard, and black; his spirits and pulse also were considerably relieved. The eau de luce was now omitted, but the brandy was continued every hour, until twelve o'clock at noon on the 28th of October, when the stiffness and tumour in the axilla had disappeared; the arm was still swelled, but was softer, and less painful. The brandy was omitted: at night he took six grains of Dr. James's powder. On the 19th of October the arm was less, softer, with little or no pain: a blister was formed and burst on the back of the hand, which discharged three ounces of black fœtid pus. On the 20th an abscess burst on the hand, in the same situation as the blister, which discharged a large quantity of a fluid having an offensive smell. He was directed to take a drachm of Peruvian bark in port wine every two hours. On the 22d the swelling was gone, but the discharge was considerable. From this time the man gradually but slowly recovered, with the loss of the use of the forefinger, which remained permanently extended, and some of the other fingers were affected in a less degree.

In this case the swelling of the arm was slower in coming

on, and less extensive: the pain running up to the axilla, which preceded it, was mistaken for the effect of absorption.

In the year 1782, while in the island of St. Lucia, I made the following experiment.

A spotted dark-coloured snake, about two feet in length, having the poison fangs on each side double, with the corresponding surfaces grooved, so as to form a canal for the poison, was put into a square tin box, open at the top, in which a half-grown rat was confined. The rat expressed great terror, and remained crouching in one corner of the box, with its eyes fixed upon the snake, who lay coiled up at some distance. They were allowed to remain a few minutes in this situation. I then raised one end of the box, which caused the snake to slide along the smooth surface, till it came in contact with the rat, which it immediately bit. The rat died in a minute after the bite. I removed it immediately from the box by means of a pair of long forceps. The wounds made by the fangs were marked by two specks of blood immediately below the shoulder blade. On dividing the skin with a scalpel, the cellular membrane under it was found entirely destroyed; the muscles were detached from the ribs, and from a small portion of the scapula. The parts immediately surrounding the bite were exceedingly inflamed. The appearances very much resembled those produced on the muscles of a dog's thigh by the application of white arsenic, in consequence of which death ensued in about sixteen hours.

Fifteen hours after the death of the first, a second rat was bitten by the same snake. This rat was much irritated, and bit the snake in the neck so violently, that the latter

died in about ten minutes. The rat continued very lively for about six hours, and then died. On examination after death, the bite was found to have been inflicted on the left side of the navel, and the abdominal muscles at that part were in the same state as in the other rat, but in a less degree.

It appears from the facts which have been stated, that the effects of the bite of a snake vary according to the intensity of the poison.

When the poison is very active, the local irritation is so sudden, and so violent, and its effects on the general system are so great, that death soon takes place. When the body is afterwards inspected, the only alteration of structure met with, is in the parts close to the bite, where the cellular membrane is completely destroyed, and the neighbouring muscles very considerably inflamed.

When the poison is less intense, the shock to the general system does not prove fatal. It brings on a slight degree of delirium, and the pain in the part bitten is very severe. In about half an hour, swelling takes place from an effusion of serum in the cellular membrane, which continues to increase with greater or less rapidity for about twelve hours, extending during that period into the bite; the blood ceases to flow in the smaller vessels of the swoln parts; the skin over them becomes quite cold; the action of the heart is so weak, that the pulse is scarcely perceptible; and the stomach is so irritable, that nothing is retained in it. In about sixty hours these symptoms go off; inflammation and suppuration take place in the injured parts; and when the abscess formed is very great, it proves fatal. When the bite

has been in the finger, that part has immediately mortified. When death has taken place under such circumstances, the absorbent vessels and their glands have undergone no change similar to the effect of morbid poisons, nor has any part lost its natural appearance, except those immediately connected with the abscess.

In those patients who recover with difficulty from the bite, the symptoms produced by it go off more readily, and more completely, than those produced by a morbid poison which has been received into the system.

The violent effects which the poison produces on the part bitten and on the general system, and the shortness of their duration, where they do not terminate fatally, has frequently induced the belief that the recovery depended on the medicines employed; and in the East Indies, eau de luce is considered as a specific for the cure of the bite of the cobra di capello.

There does not appear to be any foundation for such an opinion; for when the poison is so intense as to give a sufficient shock to the constitution, death immediately takes place; and when the local injury is of sufficient extent, the patient also dies; while all slighter cases recover.

The effect of the poison on the constitution is so immediate, and the irritability of the stomach is so great, that there is no opportunity of exhibiting medicines till it has fairly taken place, after which there is little chance of beneficial effects being produced.

The only rational local treatment to prevent the secondary mischief, is making ligatures above the tumefied part, to compress the cellular membrane, and set bounds to

the swelling, which only spreads immediately under the skin, and scarifying freely the parts already swoln, that the effused serum may escape, and the matter be discharged, as soon as it is formed. Ligatures are employed in America, but with a different view, namely, to prevent the poison being absorbed into the system.

SECT. IX.

Hints on the Mode in which the different Ingredients are separated from the Blood in the living Body, hitherto considered as Secretions.

These experiments and observations were suggested by Sir Humphrey Davy's brilliant discoveries respecting the powers of electricity in separating bodies into their constituent parts, which had always been considered as simple substances; leading me to believe that the blood by the same means might be decomposed, and the nature of the secretions ascertained. They were made under the instructions of the members of the Animal Chemistry Society, and were carried on by their joint labours. They, better than any other physiologists, from their knowledge of the different branches of chemical science, were fitted for this undertaking. These hints were laid before the Royal Society in the year 1808, at which time Dr. Wollaston's observations on this subject had not appeared in the Philosophical Magazine, nor had Dr. Young's Syllabus been published; Dr. Barzelius, it is true, had published, in the Swedish language, a work on Animal Chemistry, in which he brings forward an opinion that the secretions in animal bodies depend upon the nerves, although unable to explain how the effect is produced; but this work I never saw, nor was in any way acquainted with its contents, till after the following observations had been read before the Royal Society.

Some animals being supplied with an apparatus for their defence and for catching their prey, which resembles the Voltaic battery in collecting, and then discharging the electricity thus accumulated, furnishes two important facts: one, that such a battery can exist in an animal body; the other, that nerves are necessary for the management of the machinery, since the electric organs of the torpedo and electrical eel are supplied more abundantly with nerves than all the other organs of the animal taken together, in the proportion of twenty to one.

The nerves, from being made up of a number of fibres, a structure quite different from that of electrical organs, are not fitted to become a Voltaic battery of high power, but Sir Humphrey thought them adapted to receive a small electrical power.

That nerves and muscles can be so arranged as to collect and communicate electricity like a Voltaic battery, is ascertained by the well-known experiment of cutting off the two hind legs of a frog, laying bare the crural nerves, and applying one of them to the exposed muscles of the other limb, and then when the circle is completed, by raising the other crural nerve with a glass rod and touching the muscle of the limb to which it does not belong, the muscles of both are excited to contraction.

There are several circumstances in the structure of the nerves, and their arrangements in animal bodies, which do not appear at all applicable to the purposes of common sensation, and whose uses have not even been devised. Among these are the plexuses in the branches of the par vagum, which go to the lungs, and in the nerves which go

to the limbs; the ganglions which connect the nerves belonging to the viscera, with those that supply the voluntary muscles, and the course of the nerves of the viscera, which keep up a connection among themselves in so many different ways.

The organs of secretion are principally made up of arteries and veins; but there is nothing in the different modes in which these vessels ramify, that can in any way account for the changes in the blood out of which the secretions arise. These organs are also abundantly supplied with nerves.

With a view to determine how far any changes could be produced in the blood by electricity, at all similar to secretion, Mr. William Brande, who has begun his career in animal chemistry with so much success, made the following experiments, in the suggestion of which Sir Humphrey Davy afforded him every assistance. They were made in the year 1809.

Experiment I.

The conductors from twenty-four four-inch double plates of copper and zinc, charged with a very weak solution of muriatic acid, were immersed in four ounces of blood, immediately on its having been withdrawn from a vein in the arm. The temperature of the blood was kept at 100° during the experiment. The apparatus was so constructed as to admit of the products of the negative and positive

wires being separately collected and examined. When the electrisation had been carried on for a quarter of an hour, all action seemed to have ceased. The blood which had surrounded the negative wire was of a deep red colour, and extremely alkaline; that surrounding the positive wire was slightly acid, and of a brighter hue.

In this experiment, the coagulation of the blood was not materially affected by the electrical power alluded to.

Experiment II.

Finding it necessary to submit perfectly fluid blood to the action of the electricity, the following experiment was undertaken with a view of keeping it the longest possible time in that state.

A deer having been pithed, the abdomen was immediately opened into, and a length of about four inches of a large vein of the mesocolon was detached from the neighbouring parts. Two small platina wires, connected in the usual way, with forty three-inch double plates, were inserted into this detached portion of vein, and secured by ligatures having their points at a distance of about one inch from each other. The communication with the battery was kept up for one quarter of an hour, a third ligature was then tied in the centre of the detached vein, in order to cut off the connection between the positive and negative ends. On removing the portion of the vein included by the ligatures,

and containing the conductors, it was found that the gaseous products had forced out nearly the whole of the blood, at the part through which the wires were inserted; alkaline and acid matter were readily detected, but no new product could be discovered.

Finding the coagulation of the blood an insurmountable obstacle to the long-continued electrical action, the serum only was employed in the following experiments.

Experiment III.

The conductors from twelve four-inch double plates, highly charged, were brought within two inches of each other, in some recent serum of blood, obtained free from the colouring matter, by carefully pouring it off from the coagulum. Coagulated albumen was rapidly separated at the negative pole, and alkaline matter evolved at the positive pole, a small quantity of albumen was gradually deposited, and litmus paper indicated the presence of acid. These are the effects produced by a high electrical power upon serum.

Experiment IV.

Was undertaken to ascertain the effect of a low power: a battery was employed, consisting of twelve four-inch double plates of copper and iron. In this case, there was

at first no appearance of coagulation at either pole; in five minutes the positive wire became covered with a film of albumen, and in fifteen minutes a filament of about a quarter of an inch in length was seen floating in the fluid, and adhering to the same wire.

Experiment V.

Two small platina cups, connected by a large quantity of cotton well washed, and each containing one ounce of serum, were rendered positive and negative, by thirty double three-inch plates *very weakly* charged. The process was continued during twenty-four hours. This power had not been sufficient to produce coagulation at the negative pole. On examining the fluid in the negative cup, it was found to consist principally of an alkaline solution of albumen.

The fluid in the positive cup was rather turbid, it reddened litmus, and was slightly acid to the taste. On standing it deposited a few flakes of albumen. When evaporated, it afforded saline matter, with excess of acid.

By these experiments it is ascertained that a low negative power of electricity separates from the serum of the blood an alkaline solution of albumen; that a low positive power separates albumen with acid, and the salts of the blood; that with one degree of power albumen is separated in a

solid form, with a less degree it is separated in a fluid form.

From these facts, the following conjectures are hazarded:—

1. That such decomposition of the blood by electricity may be as near an approach to secretion as could be expected to be produced by the artificial means at present in our power.

2. That a weaker power of electricity than any that can be readily kept up by art may be capable of separating from the blood the different parts of which it is composed.

3. That the structure of the nerves may fit them to have a low electrical power, which can be employed for this purpose, and as such low powers are not influenced by imperfect conductors as animal fluids, the nerves will not be robbed of their electricity by the surrounding parts.

4. That the discovery of an electrical power, which can separate albumen from the blood in a fluid state, and another that separates it in a solid state, may explain the mode in which different animal solids and fluids may be produced, since, according to Mr. Hatchett's experiments, albumen is the principal material of which animal bodies are composed.

5. That the nerves of the torpedo may not only keep the electric organ under the command of the will, but charge the battery, by separating the fluid, the application of which, between the plates, is necessary for its activity.

6. As albumen becomes visibly coagulated, by the effect produced from twelve four-inch double plates of copper and iron, a power much too low to affect even the most delicate

electrometer, may not this be occasionally employed with advantage as a chemical test of electricity, whilst the production of acid and alkali, affected by still inferior degrees of electricity to those required for the coagulation of albumen, may likewise be regarded as auxiliary tests on such occasions?

As these facts and observations throw some light upon the principle of secretion, it will be an advantage to medical science that they are laid before the public, as hints for future inquiry.

CHAPTER III.

On the Brain and Nerves.

In the third volume I brought forward a body of evidence upon the structure of the human brain, and the effects that are produced on its functions by injuries committed upon the different parts of the organ, but found myself so deficient in many points of information respecting this very important inquiry, as to be unable to make any material progress in its physiology.

I have, therefore, ever since that period, continued to seek for further information, and have allowed no opportunity of ascertaining new facts to escape me; discarding all theoretical opinions which do not stand the test of experiment, — the only test which should be relied on in all cases in which it can be resorted to.

SECT. I.

On the Structure of the Brain.

Several attempts have been recently made to dissect the brain, but as the gelatinous materials of which it is principally composed became an unsurmountable obstacle to this mode of examination, anatomists, by the aid of chemistry, brought it to a consistence better fitted for their purpose, by means of mineral acids and other agents; and when it was rendered sufficiently solid they traced the different directions of its fibres, and represented these in magnified diagrams, for demonstrating the appearance of the parts to their pupils.

Such diagrams have been well received in lectures on the brain; but I readily detected the great error into which the anatomists who adopted this mode of preparing the substance of the brain for dissection had fallen, by overlooking two circumstances of the greatest importance; the one, that the different parts of this important organ have in their natural state different degrees of consistence; the other, that when substances are changed, by whatever means, from a less to a greater degree of density, they must occupy a smaller space, and will be found to crack or subdivide; and the appearance of these cracks is so regular, as readily to be mistaken for natural structures; and there will always be a large fissure at the union of any two portions between

which there is a considerable difference in density, even when the same fibres are passing without interruption through both.

From these observations it is evident that no correct notion of the structure of the brain can be otherwise acquired, than from an examination of the parts in a natural state, which can only be made when they are highly magnified, and this should be done immediately after death. Having satisfied myself upon this point, I prevailed upon Mr. Bauer to make such an examination by taking a thin slice of brain, including both the medullary and cortical substance, cut off from the organ a few hours after the person expired, and immersed in distilled water, which by dissolving the jelly forming the surface of the section, prevented the transparency of the parts from being diminished. This drawing, which I have annexed, shows distinctly that the same fibres are continued on from the circumference of the cortical substance into the medullary structure, following the same direction through both of them; while, in the diagram, the course of these fibres is represented as being in the cortical part at right angles to those in the medullary, a large crack having taken place between the dense cortical structure and the more gelatinous medullary substance.

SECT. II.

On the Varieties that have been met with in the Structure of the Human Brain.

The brain has been considered as the most constant and uniform in its structure of all the organs of an animal body. I have, however, met with the following deviations in this respect in the human brain: the falx of the dura mater, in one instance, was entirely wanting, and the two hemispheres appeared to be one uniform mass, the cortical substance not dipping down, but going over this mass in the same way that it usually does the separate hemispheres. I had no opportunity of ascertaining what effect this peculiar deviation from the ordinary structure of this organ produced during life.

The termination of the lateral ventricles I have in several instances found to occur an inch, and even an inch and a half, short of what is usually met with, although all the other parts were naturally placed.

The posterior process of the lateral ventricles, which is commonly curved inwards towards the middle line, and gradually contracted, I have found, in some instances, to pass directly backwards, and exceedingly small, in others, swell out into a large cavity. It appears to vary in shape and size more than any other part of the organ; and this deviation seems to be the consequence of there being

an increased quantity of water, for which it forms a reservoir; so that I am inclined to believe it is for this purpose it was originally formed.

The middle soft union between the thalami nervorum opticorum, called the soft commissure, I have found wanting almost in every tenth brain, that I have examined.

The layers forming the septum lucidum are very frequently separated, and water contained between them.

One of the mammillary processes of Willis in one brain was wanting.

The corpora olivaria are sometimes united together, but not unfrequently there is between them medullary bands, called corpora pyra midalia; and when these are wanting this name is given to the outer edge of the medulla oblongata.

The union of the optic nerves, as well as the nerves themselves, while within the dura mater, are enveloped by the pia mater, continued from the basis of the brain.

Mr. Weir, the oculist, met with one instance of ophthalmia, in which there was a small pellucid bladder upon the union, which was probably only an aqueous fluid collected under the membrane (compressing the nerves). The membrane itself had considerable strength.

SECT. III.

The Organization of the Human Brain destroyed by the Act of freezing.

The earliest doctrines I had been taught having been that animal life is dependent upon the blood, and having already detailed experiments which prove that freezing the blood of a part does not destroy life, since a rabbit's ear recovered itself after being completely frozen, I adopted the following favourite opinion of Mr. Hunter, that could an animal, and even man, be exposed to a degree of cold so intense as to be suddenly frozen, he would when thawed remain alive. When the expedition was fitted out to explore the arctic circle, I was very sanguine that this experiment would be tried upon animals, so as to set the question at rest.

Finding upon the return of the ships that this had not been done, I mentioned to Sir H. Davy my regrets. He said immediately we have means of producing cold sufficient to freeze a frog; let us try the experiment: I will give my assistance.

This was accordingly done at the laboratory of the Royal Institution, in the following manner. Two healthy frogs were chosen of nearly the same size, and each of them was separately inclosed in tin-foil, and immersed in a cold mixture of salt and snow cooled to zero. They were allowed

to remain at that temperature for four hours, one of them was then taken out and split through the brain and heart, which were both completely frozen. The other frog was taken out of the mixture and allowed to thaw gradually in snow: when it was completely thawed it was found to be deprived entirely of life; and upon examining the state of its brain, it was found to have been dissolved, and nothing remained but some gelatinous matter floating in a watery fluid.

This experiment was made by Mr. Farrady, under the direction of Sir H. Davy, in my presence.

That the brain should be dissolved in the act of freezing was an effect which I had not in the least contemplated: but the fact was no sooner ascertained than I determined to repeat the experiment upon the human brain; and after the portion of brain had been thawed to compare its appearance in the microscope with the magnified drawing Mr. Bauer had made of the human brain in a natural state.

This experiment was also made in the laboratory of the Royal Institution, in my presence, by Mr. Farrady. A portion of brain was taken from a person a few hours after death, of nearly the same shape and size as the subject of Mr. Bauer's drawing of the brain in a healthy state. It was wrapped up in tin-foil, and weighed in the balance of the Royal Institution before it was immersed in a cooling mixture and frozen. It was then allowed very gradually to thaw, and afterwards the tin-foil covering was unfolded, and a quantity of watery fluid surrounding the portion of brain

was allowed to drain off; the tin-foil was again folded up, and upon being re-weighed it had lost twenty per cent.

This portion of brain was put into the field of the microscope: the changes it had undergone in the act of being frozen show that its organization had been completely destroyed. This mode of decomposing the brain proves, in the most satisfactory manner, that the transparent elastic jelly soluble in water is the most important ingredient in the composition of that organ.

That the membrane of the retina consists of this jelly is proved by its being invisible during life, and being soluble in distilled water after death.

SECT. IV.

The Functions of the Brain elucidated by the Effects produced by Disease on the Organ.

We have already gone largely into this subject, where opportunity was afforded, after death, of ascertaining the degree of injury the organ had sustained; and I have since collected many instances in which, from the patient's recovery or continuing for many years to survive the effects of the mischief that had been done to the organ, we have been able to ascertain the mode in which the functions were restored, and the degree to which the recovery took place. These histories afford very important instruction in the present enquiry into the human intellect.

Before I mention any fresh cases of this kind, I feel myself called upon to take notice of two, whose histories are begun in the third volume, while both individuals were alive: since that time I have had an opportunity of knowing their termination, and shall now insert the occurrences that took place that are worthy of notice.

The history of the case of hydrocephalus was carried down to the year 1822: the person died in 1824. In the intervening period no change took place in his habits or intellectual faculties, except that his mental powers continued to deteriorate; but he was sensible to the last; and it is remarkable, that his amiable feelings never forsook

him. For although he suffered much from his own infirmities, his last moments were employed in the endeavour to console his mother, whose only distress arose from seeing him suffer.

The other case of the boy, who at five years old had lost half an ounce of the substance of the brain, I have lately been able to trace to its conclusion. To do this was so much an object in my mind, that I persevered in the enquiry for thirty years, at the end of which I procured an interview with his brother, who is a solicitor in considerable practice. When I mentioned the object of my enquiry, he expressed his willingness to comply with my wishes. Upon reading to him that part of the case I have already published, when I came to the remark that he was cleverer than his brother, he said, " No, not cleverer, although certainly very quick." The brother informed me that he died of a fever when nineteen years of age, and that he never had any headaches or other bad consequences from the loss of brain. His memory was good, his disposition mild, he had a turn for music, had a good ear, and sang with taste: he was well grown. The pieces of the skull removed were never renewed by bone, but only membrane, which from its yielding nature made no pressure on the brain.

The only peculiarities I could learn from his brother were, that he both walked and sung in his sleep, and at these times was very docile, submitting to be led to bed, but never awoke while conducting him there. The loss of a portion of brain may have prevented the faculties during sleep from being completely at rest, although there was no consciousness at the time of the exertions which he made.

The symptoms brought on by accidental violence in the following case appear to me to deserve consideration in the present discussion. A young man, twenty-five years of age, on the 11th of December, 1797, of a very irritable and affectionate disposition, fell from his horse. He pitched upon the right side of his head, and although not senseless, was wild and stupefied. It was eleven o'clock in the night. He lay for about four hours in a hard frost, then crawled to a cottage using his left arm, imagining the side injured to be paralytic. His scalp was cut, and bled very copiously. He was not bled at the arm. He had some years before attended a very particular friend, who, in consequence of a similar fall, lost his senses and died. This remembrance, probably, made him believe himself paralytic, since it is not usual for the injured side to be so affected. Retchings came on, with violent fever, about twelve days after the fall. The wound was purposely kept open: the retchings were accompanied with intense pain in the head, which was relieved by the application of leeches, but this was not repeated. In February the pains in the head brought on fainting. On the 31st of March he was well enough to do his duty as an officer in the army, could walk eighteen or twenty miles a day, and keep himself warm in the severest weather. His report on that date is, "My faintings and violent pains are banished. I have the appearance of health, and have dined several times in company with impunity. My giddiness continues, but in a much less degree; but my nights are dreadful: a thousand feelings, not to be explained or accounted for, affect me, and at those times I suffer dreadfully, and can give my attention

to nothing. They now rarely come on, except in the night."

This is a state of mind bordering upon insanity, in consequence of inflammation of the brain after the concussion it received in the accidental fall, and by care and management may go off entirely; since, in the course of less than four months, the symptoms had lost considerably of their violence and frequency of recurrence.

The following case of diseased cranium is very instructive: — Captain A——, at the age of fifteen, in 1798, went through two salivations in succession, occupying a period of several months on account of a venereal complaint. Six months after being well, contracting a fresh chancre, he took mercury for one month. At seventeen years old he went to Madras in the East Indies. In 1800, when twenty, he again, on account of fresh complaints, went through a salivation. While taking mercury his horse fell with him, and in the fall his helmet was broken to pieces, but he was not conscious of the head having sustained any injury. His general health became so much impaired, that it was with difficulty he got through his military duty. The war being at an end, he performed a journey of 700 miles, in the hottest season of the year, to try the effect of sea-air for the recovery of his health. During the journey his eyes became so much affected, that he could not bear strong light, and he was liable to head-aches; a swelling appeared upon the breast-bone, and stiffness in the muscles of the neck. These were considered the effects of rheumatism, and applications of salt were tried for their relief without benefit. On his arrival at Madras he was quite

exhausted in his strength, his neck was immovable, and his head-aches greatly increased. He was now put upon a course of nitric acid, under which, and the sea-air, his health improved. Within a month a swelling appeared on his forehead, like a red spot, yielding to pressure. This was considered as a venereal affection of the skull, and he went through a course of mercury for four months, at the end of which all his symptoms were increased. In 1804 he was sent to England: the voyage improved his health; and the swelling in the forehead, which had broke, healed up. He was advised exercise on horseback and country air, but no medicine. His health became more impaired; and he had occasional swellings on his forehead and over the right eye, the vision of which became imperfect; but all these symptoms went off. In 1807, he returned to Madras, and on his arrival was nearly in the state he left it three years before. In a month after his arrival the complaints in his head greatly increased; but these symptoms being considered to be the effect of mercury, it was hoped that they would wear off, and nothing was done by medicine. In three months he went 400 miles to join his regiment. During the journey his head became worse, and attended with external pain and dulness of hearing, and, on one occasion, for a few hours, quite deaf; he very soon became unfit for duty, and returned to the coast, and remained there four years, without making any advance towards a recovery. Early in 1811 he embarked for England. During the voyage the pains of the limbs required the free use of laudanum: they were attended with œdema, although his general health was improved; but his view of distant objects had become

indistinct, although he could read small print. Dr. Baillie was consulted; but the state of the head was not mentioned to him, as at this time it was not troublesome. An alterative course of mercury was pursued without benefit, then Cheltenham, then Bath. While at Bath he appeared better; but strong lights were more offensive, and his hearing very dull. He then returned to London. The journey brought back the swellings on his head, which subsided on taking a few mercurial pills. This was the beginning of winter. His health gave way: no appetite, no sleep; his strength exhausted. In this state he returned to Bath: the motion of the carriage affected his head so violently as sometimes to take away the power of speech. At Bath emetic medicines were used, and blood was taken from the arm, which last gave relief. He was confined to bed, and his life despaired of. They said he had a paralytic stroke (this, however, was an epileptic fit, in which he remained insensible for several hours). He was cupped and bled at the temporal artery. In about the eighth week of his confinement he became almost entirely blind and deaf. It was singular that upon one occasion he all at once recovered his hearing, but in a few days lost it again. The medicines administered are not known; but as his head was enormously swelled, his tongue became black, and a violent purging ensued: there is reason to believe they were mercurial. He saw light at intervals: he recovered slowly from this state of exhaustion, and was able to get out of bed. His deafness was diminished; he began to distinguish objects. In this state he returned to London. The deafness was treated as a separate complaint, the

blindness as another; but none of the means employed were of any use.

On the 4th of June, 1814, I first saw him. He was so extremely deaf, that only one or two persons could make themselves understood. There were seven or eight small tumours on his forehead and different parts of his head; the scalp had no motion upon the skull; the right eye was pushed forward so as to project considerably; and he had no sight in it. The features of the face were much distorted; he had violent head-aches, short interrupted sleeps, was confined to his apartment, and had an idiotic stare with his left eye; but could not distinguish objects, and could only make out the outline of any person when the light was weak, and he stood with his back to it.

I stated that the deafness and loss of sight were only symptoms of pressure upon the brain; that these tumours arose from a diseased thickening of the cranium, from the effects of mercury in a hot climate; and that there was a thin fluid thrown out between the cranium and scalp, and also between the cranium and dura mater; and the only mode of obtaining relief was cutting down upon the skull, and procuring an exfoliation of both tables.

I cut down upon one of the largest tumours, the base of which was nearly an inch and a half in diameter; the pericranium was thickened; the parts were very vascular, and bled freely: the surface of the skull was exposed and denuded to the extent of an inch square. He experienced immediate relief in his head, and in ten days the general thickening, and all the tumours, had so much subsided, that his hat had become too large for him. His vision with

the left eye was also improved. In about fourteen days the granulations began to cover the bone, and there was no disposition to exfoliation, the skull having a supply of blood from within: I was therefore induced every other day to destroy the granulations with lunar caustic, and also rub the exposed bone with it. In about six weeks, by this means, I got an exfoliation of the upper table. All this time his health, spirits, and the appearance of his face improved; the right eye was less prominent, and he saw light with it. This was the middle of July. The diploe exposed was exceedingly vascular, and I applied the caustic to it, which gave pain. In this way I brought off the exfoliations, and early in November I got through the inner table, but only by a small aperture, and by using considerable force. The immediate effect was the sensation of the whole skull giving way, and his nearly fainting away, with a flash like lightning before his eyes. When he recovered himself, he felt lighter and more himself than he had hitherto done. During the last three months, the head was often so hot as to require sponging with cold water, and to have cloths wrung out of cold water applied to it, which always gave relief. When he lay down, there was a rush to the head, principally in the line of the longitudinal sinus, and his dreams were very disturbed. The sides of the hole through the skull were nearly an inch thick. For two or three days there was a limpid fluid passed out of the wound, in such quantity as to require the lint being changed two or three times a day, and he felt a proportional relief. He improved in the sight of his left eye, and saw with the right; but the object seen by the right eye was not on the same plane with that seen by the left, but always a great deal higher; at first

so much so, that the objects with the two eyes could not be seen at the same time, so great was the distance of the two images. He could not hear a bell directly behind him, or the sound of a large organ. About the middle of November, a quarter of an inch square of the under table was brought away. He now was able to read for a few minutes common print, and ascertained the progress of the improvement of his sight, by finding out objects in some prints in the room which he had not seen before. The agitation upon the nerves produced by bringing this piece of bone away, made me resolve not to make further attempts, and as he had been during my attendance confined to one room, I proposed his leaving it; but this he could not be prevailed upon to do. About the 24th of November, in consequence of syringing his right ear, an attack of erysipelas came on, which affected the whole head and face, and was attended with delirium, but in a fortnight subsided. From this time the progress of recovery was slow, but uniform, and in February 1815 he went into the country, and walked about the garden, and used the shower-bath, all which improved his general health. Towards the end of March the wound in the scalp was entirely healed. He now amused himself by reading novels, but could not be persuaded to come into the society of his family. In May he began to ride on horseback, and found that he could see his way better than he expected, and rode twelve miles out, and returned home again without fatigue. He played bowls, and his sight had become good enough for the common purposes of life,— writing, reading, and knowing those he was intimate with. On the 30th of July he set off for Scotland, to bathe at

Leith. His hearing was not so much restored as his sight; and this arose from an over-anxiety to hear, which brought on a degree of nervousness that agitated him, and made him for the moment hear worse; but he gradually recovered confidence in himself, and was in perfect bodily health. In 1816, he was living at Bath, where he married, played cards, and went out to parties. In 1826 he sold off his patrimonial property, and proposed going to reside abroad.

I shall state a third case, in which the symptoms of concussion were violent and long continued, but gradually went off, the different parts of the brain recovering their functions.

A surgeon's mate fell from the quarter-deck of the Ocean, a first-rate man of war, on the evening of the 6th of February, 1780. He remained insensible till next morning: he was bled at the arm, and lost from his left ear sixteen ounces of blood: on the seventh, had great sickness and giddiness, lost sixteen ounces of blood from the arm, his pulse quick and irregular: eighth, pupils of both eyes dilated, the left most so, delirious at intervals, restless, pulse irregular: on the fourteenth, better: fifteenth, in a state of high excitement, such as intoxication might produce, and staggered in walking: sixteenth, head-ache very violent, much fever, lost eight ounces of blood, took opening medicine: nineteenth, his face swelled, a discharge from the left ear, more restless at night: twentieth, was in a state of dozing: twenty-fifth, head-ache violent, relieved by bleeding and blistering: March 3, head-ache less, unable to walk straight. It was a year and a half before he was sufficiently recovered to return to his professional duty. He afterwards continued well.

SECT. V.

Affection of Injured Nerves.

A GENTLEMAN, thirty-five years old, rode against a post with some violence on January 12. 1816; it struck him below the outside of the knee. He was in great pain, confined for eight days: on the seventeenth, moved the leg slightly: on the twenty-second, he was allowed to leave the horizontal posture. On bending the knee, it immediately brought on pain, and violent affections of the nerves of the leg. I saw him on the twenty-fifth standing on crutches, the knee a little bent, in great agony. When placed in a horizontal posture, he was easy, the knee being straight. He lay all night with a board behind the ham, to prevent motion; had no return of pain: next day wrenched the knee in getting on the bed-pan, but no pain followed; yet in the evening, bending the other knee, the pain came into the same nerve of that side: this, after lasting several hours, subsided. The following night, the pressure of the bed-clothes on the great toe of the injured side brought on pain in the second joint: this went off after several hours. Took thirty drops of laudanum, which brought on head-ache, but had no pain. He took ten grains of Dover powder every night. On the 21st of February, able to walk without crutches: a splint used, to guard the ham.

A gentleman, aged fifty-six, in consequence of anxiety ten years before, had uneasy feelings at the præcordia, and occasional spasms in the heart. These increased upon him, and a second source of anxiety, ten years after the first, aggravated his symptoms; and when the spasms subsided, they now left a gnawing pain in the left arm, from the shoulder to the elbow, extending occasionally to the fingers, obliging him to wear his arm in a sling. This was not of long duration, but increased till his death in frequency. They never produced fainting nor insensibility; but the last, preceding death, was excruciating agony, and only terminated with life.

After death, the heart was unusually small, the coronary arteries ossified down to their subdivision, but not lower; the inner membrane of the aorta, from the semilunar valves to the diaphragm had a layer of ossification as thick as a wafer, broken into pieces like a tesselated pavement; some of the edges turned up, the interstices appearing ulcerated. Some of the intercostal arteries had their orifices obliterated, also that of the left subclavian. The pain in the arm greater than that in the heart.

SECT. VI.

On the Nerves of the Placenta.

Having become convinced that every animal had something analogous to brain, I was induced to believe that there was no part of an animal unprovided with nerves: this, however, required to be proved.

The horns of the fallow deer I knew to have considerable sensibility, while they remain covered with the velvet, although afterwards, when arrived at their full growth, so as to be employed both as a weapon of attack and of defence, no such sensibility exists: the nerves then belonging to such horns must be employed for the purposes of their growth only. Upon examining the parts between the velvet and the substance of the horn, Mr. Bauer found the nerves to be very abundant, equally so with the arteries.

The only organized structure, therefore, in which any doubt remained of the existence of nerves, was the placenta; and Mr. Bauer very kindly complied with my request, in examining its structure in the microscope in search of them.

As the nerves of a part that has been long kept, whether in brine or spirit, acquire a yellow colour, which admits of their being more readily detected, I supplied Mr. Bauer with the placenta of a seal, brought from the arctic circle, preserved in brine, and which had the advantage of having

the arteries and veins injected with different-coloured wax; and as in that animal the umbilical vessels are not twisted together, the nerves would be more easily exposed, and detected. It will appear from the annexed drawing, that they were readily recognized, and are conspicuous surrounding the umbilical arteries; and that they were demonstrated even in the uterine portion of the placenta. While engaged in examining the seal's placenta, I received from Sir Stamford Raffles the placenta of the tapir of Java; and as in that animal the chorion has its surface so formed as to answer the purpose of the placenta, the nerves were very distinctly traced from the umbilical chord to the spongy structure of that membrane.

To render this discovery complete in all its parts, I got from Mr. Brookes, teacher of anatomy, a well-injected specimen of human placenta, that had been forty years in spirit, and while Mr. Bauer was engaged in examining this placenta, I found the nerves of the chord lying in the space between the arteries and the vein: he very readily discovered them in the placenta itself.

This discovery places the placentular circulation in a new point of view, since, from the known influence of the nerves on the blood-vessels, it is reasonable to believe, that during life there are branches of communication between those of the uterus and fœtus, although too minute to be explored in the dead body. The erection of the penis cannot be produced after death by injecting the arteries, although when the nerves are excited, these smaller arterial branches give a ready passage to the blood. Having traced nerves from the fœtus to the maternal portion of the placenta, it

will add to the value of this new fact, to give some general account of the course of the nerves which supply the uterus of the mother, more especially as these are little known in the different classes of animals.

That some very important purpose is answered by the uterine nerves is evident from their number, the different sources from which they originate, and the various ganglia by which the filaments are connected with one another; and that such a complex system of nerves is required for the well-being of the fœtus in utero cannot be doubted, since they become enlarged during pregnancy.

Mr. Cæsar Hawkins has very kindly undertaken the necessary dissections for this purpose; and I shall give in his own words the distribution of nerves connected with the organs of generation of the female, in the human species, the quadruped, bird, and the frog.

" The nerves of the human uterus are supplied from six plexuses; the spermatic within the abdomen, the great hypogastric between the common iliac arteries, and from four within the pelvis, two of which are situated on each side of the uterus. All of them have the peculiar appearance of the sympathetic nerves, and are intimately connected with all the other nerves of the viscera.

" The uterine nerves in the cat, dog, rabbit, and guinea-pig, so nearly resemble those of the human uterus, that a minute description becomes unnecessary. The spermatic plexus is formed by branches of the renal plexus and the two nearest lumbar ganglia of the sympathetic nerve; it supplies the horns of the uterus, the ovaria, and apex of the urinary bladder."

The common hypogastric plexus, after having supplied the body of the uterus, gives off a large nerve of considerable length, which dips down into the pelvis, and unites with numerous branches from the second and fourth ; a remarkable plexus is thus formed which contains several distinct ganglia. It distributes nerves to the body of the uterus, the vagina, bladder, and rectum, the integuments of the upper part of the pubes, and the muscles of the inferior outlets of the pelvis. A few branches pass down to communicate with the fourth sacral nerve, where it gives origin to the pudic nerve. These nerves arise from the plexus in such a way, as to resemble the ramifications of the venæ verticosæ in the choroid membrane of the eye.

The difference, therefore, between the nerves of the human uterus and those belonging to the uterus in the quadruped, consists in the formation of only one lateral hypogastric plexus, and, consequently, in the existence of only four nervous centres in the latter. There appear also to be more ganglia in the plexiform distribution of the human sympathetic nerve.

In the seal, several large ganglia are found in the broad ligaments of the uterus.

In birds, the nerves belonging to the female organs are distributed as follows : —

The sympathetic nerve is found close to the origin of the spinal nerves, protected by the double heads of the ribs, between which it runs. The spinal nerves, that correspond to the lumbar and sacral nerves in quadrupeds, emerge near each other, and as the sympathetic nerve communicates

with each of them, and forms a ganglion immediately after their appearance from the vertebral foramina, there is an almost uninterrupted ganglion of considerable length, from which numerous filaments go off to supply the oviducts; others run upwards, and are distributed on the ovaria near the termination of the oviduct. In the cloaca a plexus is formed nearly similar to the lateral hypogastric plexus in the quadruped, which is distributed in a corresponding manner to the oviduct and cloaca: there is also a similar pudental nerve.

Fewer ganglia are formed near the aorta than in the quadruped, and scarcely any branches are sent from the common hypogastric plexus to the oviduct.

In the frog, as there is no proper sympathetic nerve, the abdominal viscera are supplied directly from the spinal nerves. These, soon after they emerge from the vertebral canal, become slightly enlarged: this can hardly be termed a ganglion. From each of the spinal nerves, in the lower part of the back and loins, a small nerve is given off, which takes a direction towards the centre of the bodies of the vertebræ, where they unite with each other, and with the corresponding nerves of the opposite side. By this union a flat nervous web is formed, which stretches across the aorta, and extends downwards into the pelvis. This is analogous to the splanchnic plexus in quadrupeds.

From the upper surface of this plexus many branches run upwards towards the intestines and kidneys, but the greater number are distributed on the ovaria.

The lumbar nerves on each side give off several branches, which pass at once into the oviduct. The last lumbar

nerves pass down upon the surface of the psoæ muscles, and near the pubes give off a branch, which takes a circuitous course towards the lateral portion of the bladder, and the extremity of the oviduct. The continuation of the aortic or abdominal plexus, in union with some branches of the sacral nerves, forms on each side of the pelvis a kind of plexus, which distributes branches to the lower portion of the oviducts.

The nerves in the frog, which correspond with those that have been described, run almost entirely in straight lines, instead of having the intricate reticulated texture of the visceral nerves in the quadruped. The ganglia are indistinct, and the fibres that compose them resemble those of muscular rather than visceral nerves.

Now that it is known, by the discovery of the nerves in the placenta, that the brain of the child is connected with that of the mother, we are led to understand the degree of dependence in which the fœtus is kept during the whole time of utero-gestation.

The small-pox being, in some instances, communicated by the mother to the child, has been considered as an extraordinary fact, and not to be accounted for; but is now readily explained, since absorption depends as much upon nervous influence as the action of the arteries.

There are several cases recorded in the Philosophical Transactions of children in utero having the small-pox. Two of these occurred in England. In both, the child took the infection the fourteenth day. One was in Jamaica. The child took the infection the eighth day: the difference of time is deserving of being recorded.

A child in utero having an ague is in itself almost a proof of the existence of nerves in the placenta. Of this kind my friend Dr. Russell, from Aleppo, states the following case.

In June, 1767, a healthy young woman, in the seventh month of her pregnancy, was attacked by a tertian fever, and the fœtus appeared to suffer a paroxysm distinct from the mother. The fits in the mother returned regularly about noon, and terminated in profuse perspiration in less than ten hours. About eight in the morning of the odd days the womb trembled with great violence, the mother also felt coldness and weight there. The coldness lasted fifteen minutes, followed by a glow of heat that continued an hour. She had in her other pregnancies found the child occasionally restless, so she did in this; but this particular affection she never but on this occasion had experienced. After the sixth paroxysm the bark effected a cure.

Dr. Russell met with a few similar instances while in Aleppo. These might be, he thought, attributed to the nervous state of the mother, but in this instance there could be no ground for such suspicion.

A child being born without a brain is not to be marvelled at, the nerves of the child being connected with those of the mother.

The immediate division of the navel-string, upon the birth of the child, being in some cases attended with hazard to the child's life, shows the accuracy of Dr. Denman's observations, who objected to such practice.

Till the nerves in the placenta were discovered, we had

no mode of estimating the influence that could be produced upon the child by the affections of the body or mind of the mother, and, therefore, the instances that have occurred were considered as idle stories, or occurrences for which no satisfactory reason could be assigned.

With the following well-authenticated instances of this kind, I shall close what I have to state upon this subject.

A mare was covered by a quagga, the wild-striped ass of Africa, and had a mule striped like the father. This happened in Scotland. The mare was brought to England, and never again saw the quagga or his offspring. She was covered by a Persian horse, by whom she had in succession three foals, all marked like the quagga, although less strongly than the mule. This fact is put upon record by Earl Morton, in the Philosophical Transactions, and portraits of all the individuals are preserved in the Royal College of Surgeons in London.

A lady, while pregnant with her tenth child, all the others free from blemish, was robbed in the dusk of the evening by a man with a hare-lip. She did not for several days recover from the fright. The child was born with a hare-lip. I was called upon to perform the operation. Knowing the mother to be very nervous, every previous step was taken to insure success; but the child cried incessantly from the time it was performed, and died on the third day.

A lady of a nervous habit, who had had several healthy children, during a state of pregnancy, upon opening a door, was surprised by the Newfoundland dog, with which she was familiar, suddenly in playfulness putting his two fore paws upon the sides of her belly. The child, in all other

respects well formed, had two claret marks where the paws had been applied. These I afterwards removed.

A lady in early pregnancy was frightened by a sailor with one arm, who came as a beggar, and uncovered the stump: the child was born without an arm.

A lady near her time had a similar fright, and was rendered unhappy lest the child should suffer from the alarm she had received. The infant was perfectly formed; but her next child, born after all remembrance was gone of what had happened, was born with one arm.

An Italian woman, twenty years of age, when by her reckoning three months gone with her third child, was travelling in a caravan with the baggage of the Duke of Wellington's army on the Continent. In the middle of the night, in a violent storm, when she was fast asleep, a monkey, that had been chained on the top of the caravan, in its fright found its way into it, and, as the warmest birth it could find, got under her loins. Half asleep, she put her hand down to scratch herself, but scratching the monkey it bit her fingers, and threw her into fits. She did not miscarry, but went her full time. The child when born only weighed one pound, and measured seven inches in length. It was reared with difficulty, and was carried by its parents to Ireland, where it became consumptive: it was brought to London, and shown as a curiosity: it died just after it completed its ninth year. I saw it several times while alive, and it came into my possession after death. Its skeleton is preserved in the museum of the College of Surgeons in London, and measures twenty-one inches.

Upon examination of the body after death, the fontinelle

was closed. There was no fat in any part but in the sockets of the eyes, behind the balls. The uterus had not been developed beyond that containing a fœtus of four months: the bladder was distended with urine to the size of a hen's egg. As the child had never made water freely from its birth, the bladder probably had been injured at the time the monkey alarmed the mother.

On comparing the ovaria with those of an abortion at three months, they were nearly of the same size.

The child when I saw it could walk alone, but with no confidence. Its sight was very quick, much attracted by bright objects, delighted with every thing that glittered, mightily pleased with fine clothes, had a shrill voice, and spoke in a low tone; had some taste for music, but could speak few words of English; was very sensible of kindness, and quickly recognised any person who had treated it kindly. The mother has had a fifth child in Ireland, which, like her first three children, is naturally formed.

SECT. VII.

On the Structure of Ganglions, with Experiments to prove them to be the Sources from which Animal Heat is produced.

It is hardly to be believed, that there is not upon record an accurate description of the internal structure of a ganglion. The fact is, that microscopical observations, which the celebrated Loewenhook, nearly two centuries ago, by his application of them to animal and vegetable structures, brought into great vogue and general repute, after his death lost their credit with the public. Many that he had made were stated to be incorrect, and as there was no individual expert in the use of the instrument, when high magnifying powers were employed, it was very generally understood that this was an art he had carried to the grave, without communicating it to any one.

In this way the microscope fell into disuse as a means of promoting anatomical and physiological enquiries, no observer venturing to trust to the representations produced by lenses, that increased the size of the objects higher than twenty diameters. At the distance of time of a century and a half, there has risen up a person, whose knowledge of the microscope, and his experience in its use, has revived our confidence in the representations it produces. I need not say here, this person is Mr. Francis

Bauer, and that it is by his aid that I have been enabled to make out the more minute structure of the brain, and also that of the ganglion, giving the discriminating marks by which they may be distinguished from one another. I have been thus enabled to supply the great hiatus in anatomy, that prevented us from forming any correct notions of the structure of ganglions, which we now find to be intermediate between that of brain and nerves. The brain being composed of small globules, suspended in a transparent elastic jelly, nerves made up of single rows of globules, and ganglions consisting of a congeries of nervous fibres compacted together. As the globules which form a muscular fibre are of the same size in the human body as in the elephant, and all other quadrupeds that have been examined, it is worthy of observation that the globules which form the nerves belonging to muscles are smaller in the elephant than in man, and the nerves of the elephant are found by experiment to be more elastic than the human nerves.

There is no animal that does not possess a brain, spinal marrow, and nerves, although upon a small scale; but several tribes have no ganglions, as the oyster, great fresh-water muscle, and garden snail, while the insect and earth-worm are abundantly supplied with ganglions. These facts could not be discovered till the structure of ganglions was ascertained; for so nearly do brains and ganglions resemble one another in their external appearance, that even now the brain of the earth-worm is mistaken for a ganglion.

As the brain in the oyster has an appendage united to

it by two lateral nerves, the internal structure of which is the same as that of the brain, I consider it to correspond with spinal marrow in animals of more complex structure, and we accordingly find it in all those that have ganglions.

To make myself more clearly understood upon this subject, I must refer to the annexed drawings.

This distinction between animals without ganglions, and those that have them, led me to consider whether it might not enable me to ascertain what purpose these bodies answer in the animal economy; for if those that have no ganglions are provided with no means of preserving a heat above the temperature of the atmosphere, which we know the earth-worm, and all insects possess, particularly the bee, which can raise it to eighty-two degrees in the day, and seventy-eight in the night, it is reasonable to consider that the ganglions give this power.

The oyster, fresh-water muscle, and snail are found in summer to have the same temperature with the atmosphere, and when prevented from escaping from the cold of winter, they fall a sacrifice to it; while all animals with ganglions maintain a temperature one or two degrees above that of the atmosphere.

These facts encouraged me to prosecute this enquiry into the source of animal heat. The horns of the fallow-deer, during their growth, have a heat in June of ninety-six degrees, in July of ninety-nine and a half. To determine whether the ganglions and nerves going to the horn were concerned in keeping up this heat, it was only necessary to divide the two branches of the fifth pair of nerves, immediately above the ganglion from which they

proceed to supply the horn, and ascertain the change produced in the temperature of the horn. For this purpose I got Mr. Mayo and Mr. Cæsar Hawkins to make the following experiment on the 20th of July, 1824. The situation of the branches which supply the horn was exposed upon the head of a dead deer of full horns, which had been provided, and then a deer of two years old had the temperature of both horns ascertained by a thermometer to be eighty-four, in an atmosphere of sixty-six degrees.

The branches of the fifth pair of nerves that go to the right horn, of which there are only two, were then divided, and a hole was bored through each horn near the tip, so as to receive the bulb of a thermometer, by which the temperature of the horns might be registered.

The changes of heat in the horns are stated in the following diary.

July 21., before the experiment, both horns stood at 84°.

	Atmos.		Nerved Horn.		Left Horn.
3 Hours after the experiment	66	...	72	...	84
July 22.	64	...	69	...	95
— 23.	64	...	67	...	84
— 24.	64	...	76	...	84
— 25.	67	...	87	...	90

The animal was allowed to remain at liberty in a small paddock, with two companions, that he might be in the most natural circumstances; but, unfortunately, on the twenty-fifth he bruised the nerved horn against the

paling, and broke off the point beyond the hole in which the thermometer had been received.

Two new holes were bored further from the margin of the velvet, and the diary continued.

	Atmos.		Nerved Horn.		Left Horn.
July 26.	58	...	84	...	76
— 27.	59	...	95	...	97
— 28.	64	...	94	...	98
— 29.	62	...	95	...	97
— 30.	62	...	87	...	95
— 31.	60	...	84	...	79

The animal no longer serving any purpose respecting the object of the experiment, the nerved horn having an excess of heat above that on which the nerves were entire, the animal was killed, and the nerves examined. The cut ends were not united by regular union of nervous structure, between the two portions, but the restoration of heat was a sufficient proof that they were in some way re-united.

The beam of the nerved horn immediately above the brow antler was $\frac{3}{8}$ of an inch in circumference larger than that of the other. However much I had reason to be satisfied with the result of this experiment upon the nerves of the deer's horns, I felt it a duty incumbent upon me to repeat it, since a single experiment upon any subject is liable to fallacy, and therefore requires confirmation. In a second experiment made the following season, all the difficulties that occurred in the first were more readily surmounted, and many of them altogether done away.

There was also a facility produced by the former experience, which is in no other way to be acquired.

In the first experiment, the examination of the parts after death had not been entirely satisfactory; for the two cut ends of the divided nerve were certainly not united again, and giving our attention to them, the medium by which their sides adhered was not so nicely scrutinized as could be wished.

This second experiment was made in the year 1825, upon a deer of full head, seven years old. Before the nerves were divided, the temperature of both horns was ninety-eight and a half degrees, and that of the atmosphere eighty-four; but in twelve minutes after dividing the nerves of the right horn, the heat of the horn sunk to ninety-three.

The experiment was made on the 12th of July, at three o'clock. No holes were bored in the horns, and the heat was taken by passing over the palms a worsted elastic band, three inches broad, and the bulb of the thermometer introduced between it and the horn. The animal was confined in a stable, that he might be less liable to accident. The nerves were divided by Mr. Mayo. The changes of temperature are registered in the following diary: the heat was taken regularly every day at three o'clock.

	Atmos.	Nerved Horn.	Left Horn.
July 12. after experiment	84	93¼	99
— 13.	84	93	98
— 14.	84	93	100
— 15.	85	96	99
— 16.	86	97	99¾
— 17.	86	97	99

	Atmos.		Nerved Horn.		Left Horn.
July 18.	86	...	96¾	...	98
— 19.	86	...	97	...	99
— 20.	85	...	97	...	99
— 21.	75	...	95	...	97
— 22.	76	...	93	...	93
— 23.	74	...	93	...	93

As the object of the experiment was obtained, the animal was set at liberty, and on the twenty-eighth, being in condition, was killed for the use of the table. The velvet was still perfect; but the horns themselves had acquired considerable firmness, except at the points which project from the palm. The two horns were in all respects of equal growth.

The nerves that had been divided were carefully examined by Mr. Mayo, Mr. Hawkins, and myself, and although the space between the divided portions had not acquired a structure resembling that of nerve, it was sufficiently condensed, to leave no doubt of the nervous influence having been communicated through this medium; and although immediately after the former experiment I was doubtful of there being union sufficient for that purpose, I am now ready to admit that it had taken place.

The results of these two experiments on deer of different growth, and in two seasons, in which the temperature of the atmosphere was so different — in the one twenty degrees higher than in the other, — and the relative change in the heat of the horns being the same, renders the evidence complete.

I shall not, however, have occasion to rest my argument upon these grounds; for if once a principle is established with respect to the laws of the animal economy there can be no want of proofs of its truth, since the works of the Almighty illustrate one another in a manner not to be imitated by those of man.

The human uterus, more than any other organ, is supplied with nerves and ganglions, and during pregnancy the nerves enlarge; the temperature of the uterus should, therefore, at these times be increased. Upon enquiry among my friends who are engaged in the practice of midwifery, I found this to be actually the case, since I was told that in the act of turning a child in the womb, the heat is sometimes greater than the practitioner's hand can well bear. This I was led to know must be somewhere about 125°, as my own hand cannot remain in water heated to that degree without considerable pain.

Dr. Granville having on former occasions given me his assistance in my philosophical pursuits, very kindly, at my request, did so upon the subject of the heat of the human uterus; and on being supplied with a thermometer adapted to the purpose, he furnished me with the statements contained in the following reports.

First report: —

In a natural labour which lasted three hours. The heat of the cavity of the uterus before delivery 108°, after delivery 105°, that of the placenta 104°.

The pulsations at the wrist of the mother, in the minute, seventy, in the funis 140.

Second report: —

In a premature labour, in which the child was born alive at seven months, the heat of the uterus before delivery was 100°, after it 99°, that of the placenta 98°.

The pulsations at the wrist of the mother in the minute 60, in the funis 110.

Third report: —

In a labour that required the use of the forceps and lasted thirty-eight hours, the child alive, the heat of the cavity of the uterus, in the intervals between the labour pains, was 118°, during strong pains 120°, after delivery 110°, that of the placenta 110°.

The pulsations at the wrist of the mother in a minute 100, in the funis 120.

Fourth report: —

In a labour which lasted, from the deformed state of the pelvis, forty hours, before delivery no opportunity offered of ascertaining the temperature of the uterus; after delivery it was 115°, after the expulsion of the placenta 118°, that of the placenta 112°.

As the balls of some of those thermometers with naked bulbs are so thin as to have the mercury raised in the stem by external pressure, it is to be remarked that the thermometer employed by Dr. Granville in these experiments was not liable to that source of error.

The evidence respecting the source of animal heat, I trust, is sufficiently established to require no addition to the proofs that have been given. I cannot, however, omit mentioning the following circumstance, which, without

the support of the experiments that have been related, I should not have considered as deserving credit.

A gentleman, while attending my lectures at St. George's Hospital, given the first month of every year, told me that on the 14th of January, 1826, a married woman, whom he was to deliver, came to him complaining of such excessive heat in her belly, that her husband could not sleep in the same bed: she had at this time heart-burn and vomiting. These symptoms lasted two or three days, yielding to purgative and saline medicines. She was seven months gone with child. On the 16th of February there was a return of the same heat and other symptoms: they subsided in three days. On the eighteenth of March she was brought to bed; she stated the heat for three days in the womb to be insupportable. I had given him a thermometer to determine the precise degree, but her objections to its being used were not overcome. It must have been 120°, — as beyond that it is hardly to be borne.

That the source of animal heat is principally in the ganglions, and not the nerves, is well illustrated in the torpedo and electrical eel, whose electric organs have so wonderful a supply of nerves, but not of ganglions, and the temperature is not increased beyond that of other fishes in the parts surrounding the organs.

While engaged in this investigation, I had an opportunity, in the course of my professional duty as surgeon to the Royal Hospital, Chelsea, to ascertain the effect of tying the femoral artery, that supplies the human thigh and leg, on one side, upon the temperature of the limb when compared with that of the other, which remained in a natural state.

Before the artery was included in the ligature, the temperature of the foot was 93°, that of the other foot, which was in a natural state, was 84°. In ten minutes after the operation the heat diminished to 87°, in thirty minutes to 85°; while the left foot remained at 84°. In eight hours the left foot rose to 94°, probably from the increased quantity of blood that was carried through its branches, since next day it fell to 88°, at which it continued stationary for four days. The foot of the side on which the artery was tied in twenty-four hours sunk to 84°, but in the following day rose to 85°, at which it remained stationary till the ligature came away.

I have had many opportunities of ascertaining the temperature of the foot, after the femoral artery had been tied, in the operation for the popliteal aneurism, and found it commonly one degree lower than that on the opposite side; but the heat of the feet had never been taken before the operation, so that I was probably misled in my conclusion, and, in reality, the heat of the opposite foot had been raised above the standard, instead of this being sunk below it.

SECT. VIII.

Extracts from Professor Tiedeman's Work on the Nerves of the Human Uterus, in Confirmation of my own Observations on that Subject.

At the time I communicated to the Royal Society the discovery of the nerves being, by means of their ganglions, the source of animal heat, which is the subject of the preceding section, I had it in contemplation to request my friend Mr. Bauer to make magnified drawings of the ganglions belonging to the nerves of the gravid uterus, but could procure no means of enabling him to do so; my delight, then, may be well imagined, that under those circumstances, Professor Tiedeman presented to the Royal Society his fasciculus in Latin upon this subject, dedicated to that learned body. I found that it contained every thing I wished to see represented, or could be required, to confirm my own observations, and immediately resolved to have the plates re-engraved by an English artist, and inserted in the present work, with such extracts from the fasciculus as tended to explain and illustrate the engravings.

That I may do ample justice to the Professor who has given to the scientific world this invaluable publication, I have simply copied from the original, making no alterations, but putting what I have extracted into an English dress, and by that means diffusing the author's fame more

generally among my countrymen than it otherwise might extend.

He begins by giving a review of all that has been published by anatomists respecting the nerves, and regretting that those of the uterus have been either altogether neglected, or very incorrectly described. He confesses himself to be stimulated by this deficiency, at a time when we are daily discovering the nerves to have a greater influence on the functions of an animal body than former anatomists were ready to admit; and desirous of coming forward and giving the public a more accurate description of the ganglions and nerves of the uterus than has been hitherto published. Preparatory to such a work, his first care was to examine the uterine nerves in the bodies of women who had died soon after delivery, and then in those who had not been lately pregnant. Having done so, he began to prepare his drawings for being engraved. He says he remembered the maxim of Horace: —

> "Segnius irritant animos demissa per aurem
> Quam quæ sunt oculis subjecta fidelibus et quæ
> Ipse sibi tradit spectator."

It certainly is fortunate for science that he adopted this maxim, since he has produced two plates which stand at the head of anatomical engravings, upon a subject which, of all others, required such elucidation.

This maxim I have adopted in the present, as well as the preceding volumes, and have endeavoured to improve upon it by artfully, through the means of microscopical drawings, making parts distinct to the naked eye which in no

other way could be brought within its comprehension. After stating the different plexuses of nerves by which the uterus is supplied with branches, he says, " From this short and concise description of the nerves, it becomes clear that nature has afforded to the uterus many nerves closely compacted together, having their origin from the sympathetic.

" The whole body of the uterus being thus surrounded with the materials of which nerves are composed; and many productions from nerves being disseminated and dispersed through its substance, proves that those anatomists must err egregiously who contend that the uterus has no nerves, or only receives very small ones.

" The nerves of the uterus, like those of the heart, are soft, slender, and somewhat red. It is also to be observed, that frequent connections are to be found between the nerves and the arteries of the uterus; but these nerves and arteries are less closely and firmly held together on the surface of the uterus, binding it round, and enclosing it in a net-work, than the cardiac nerves are found to bind the coronary arteries.

" Nervous shoots, which enter the substance of the uterus, are suddenly lost, and even disappear, although examined with good magnifying glasses: they appear to be terminated or dissolved in the cellular or mucous membrane, which is the medium between the blood vessels, the lymphatics, and the fasciculi of the muscular fibres. On this account I am not disposed to agree in opinion with those anatomists who consider the smaller branches of the sympathetic nerve intended for supplying the arteries only.

" The size and abundance of the nerves met with in the uterus differs in the different periods of life. In young girls they are slender, in virgins and grown up women they are stronger and thicker. In old women they are very small, and exceedingly tender. The mass, the thickness, and abundance of the nerves of the uterus during utero-gestation are increased, as Dr. William Hunter has published to be his opinion; for in three women who died soon after delivery, which I dissected, I saw that the uterine nerves were much larger than in women who were not with child."

I shall omit giving, in this place, the Professor's proofs of the uterus possessing sensibility, as at this day they cannot be required, since no one who has considered the subject can refuse his assent to it.

" The nerves, no doubt, exercise some power over the involuntary motions of the uterus: they probably render the involuntary muscles fit to be acted on by stimuli, and, by their excitement to motion, dispose them to contract. It being certain that muscular fibres are formed during utero-gestation, it is not unreasonable to believe those motions of the uterus, by which the fœtus is expelled, depend upon and are regulated by the nervous influence.

" The nerves of the uterus seem to be given, not only for the purposes of sensation and motion, but also to regulate those vital actions of this organ termed vegetative. From what cause the nerves become larger during utero-gestation, whoever attempts to establish, I should consider engaged in a work of equal rashness, as endeavouring to explain what power the nerves have over what is called the

vegetative life of that organ; for such is the obscurity in which these matters are involved, that there is no ray of hope by which we can congratulate ourselves with the chance of success.

"Lastly, there are various agreements kept up by the nerves, between the uterus and other vital organs of vegetative and animal life, which J. V. Osiander, in a memoir, has enumerated and illustrated by drawings which I am not disposed to detail."

SECT. IX.

On the Influence of the Nerves on the Heart and Arteries.

After the brain has had its functions completely destroyed, the heart may have its actions continued so long as the nerves of respiration perform their office, and the air received into the lungs has a sufficient proportion of oxygen to supply the veins of the lungs and stimulate the heart to action. This I shall illustrate in the following case.

A gentleman who resided many years in India, and during that time was more than usually exposed to the sun, became liable to violent attacks of head-ache, terminating in a deep sleep, which lasted twenty-four hours, and left him depressed and dejected. On his return to England, in the forty-sixth year of his age, these attacks increased in frequency, not followed by sleep, but constipation of the bowels, which had its seat in the colon.

On the 5th of July, 1826, he awoke, after having had a tolerable night's rest, at five o'clock in the morning, and attempted to go to the water-closet down a stair-case of eight steps. He could not have been many minutes out of the bed-chamber, when he was found extended on the two lower steps completely insensible.

His pulse at the wrist was distinctly felt, his breathing exceedingly agitated. He continued in this state for an hour,

but the pulse stopped immediately on his being put into a warm bath. Eleven hours after his being found, I examined the contents of the skull: six ounces of bloody serum escaped while sawing the cranium. The right side of the frontal bone was as thick again as the left. In the lateral ventricle there was a tolerably firm coagulum, much larger than a common hen's egg. Under these circumstances, there is no reason to doubt, that the compression had put a stop to the functions of the brain, and the respiration had been continued under the influence of the eighth pair of nerves.

The head-aches he was liable to could now be explained, also the torpor succeeding them, as well as the depression of intellect. The attacks I consider to have begun by too much action in the arteries, increasing the pressure from the thickened bone and inducing coma, or torpor, and sleep, out of which he awoke languid, as others do from attacks of coma.

The dura mater was not only unusually vascular, but the arteries had a coating of coagulable lymph; and immediately over the superior longitudinal sinus there was a space two inches long, and one broad, so covered with vessels carrying red blood as to have the appearance of needle-work, which is an effect the sun's rays produce upon the lining of the skull in tropical climates. This would have been relieved by an issue down to the cranium on the sinciput, which I had very strongly advised to be made whenever the head-aches became either more frequent or more severe.

In the case just detailed the heart's action was kept up while the actions of respiration were continued, and no

irregularity took place in its pulsations, the nerves which supply it being uninjured.

In the following case of aneurism in the left subclavian artery, the operation of tying the artery was performed; and the nerves of the subclavian artery that are connected with those of the heart, with which they form a net-work that spreads out above and below, sending branches to the cervical ganglion, and the ganglion under the arch of the aorta, also others to the left auricle and ventricle, were included in the ligature. The injury done to these nerves, by the pressure of the ligature upon them, produced a considerable effect upon the heart's pulsations: in twenty-four hours after the application of the ligature, the pulsations of the subclavian artery above the clavicle were ninety-four in a minute, neither hard nor soft. In forty-eight hours there was violent pain where the nerve had been tied; the pulse rose to 104 beats in a minute; there was unusual irritability, and the bowels were confined, but after an evacuation had been procured, the pulsations were only ninety-eight. In seventy-two hours an acute pain was felt in the left arm, succeeded in five minutes by a rigor that lasted twenty minutes, followed by profuse perspiration, after which the pulse was only ninety-two, soft, and full. Six ounces of blood, taken from the arm, although buffy, were not cupped, but loose in texture; there was pain and numbness down the left arm, and a second rigor; the pulse the same: six ounces more blood taken from the arm were equally buffy as the last; the right elbow became swollen, attended with redness and vesication; these symptoms were followed by general restlessness, great anxiety, the pulse small, irregular,

and irritable: there was another rigor; the profuse perspiration did not go off; the man felt conscious he was dying: and in 132 hours, or five days and a half, after the operation, expired. Immediately after death the whole of the muscles and cellular membrane of the right arm to the wrist, part of the left arm, and a portion of both lower extremities, were in a state of mortification; there was a coagulum in the artery on each side of the ligature; the heart was flabby and small.

The taking up the right subclavian artery for the cure of aneurism has succeeded perfectly, and produced no unusual symptom; so that I consider the disturbed state of the nerves of the heart to have occasioned the symptoms that came on after taking up the left subclavian artery, and along with it the network of nerves surrounding the artery.

It is deserving of notice that this case and one which has been related in the chapter on the blood, are the only instances in which the coagulable lymph, after bleeding at the arm, has separated from the other parts, giving the surface a buffy but not a cupped appearance.

Is it a deficiency of carbonic acid gas that makes the coagulable lymph, when the blood is at rest out of the body, to separate in this manner? In some cases of scurvy, I have known, when blood was taken away, that the coagulable lymph lay on the surface like melted tallow, hanging over the red portion of the coagulum; but at that time I could form no notion of the cause of this appearance.

SECT. X.

Cramp, arising from Nervous Irritation, not Muscular Contraction.

This affection I have ever considered as one belonging to the fibres of the muscles, which are involuntarily thrown into action, and therefore the moment the fibres are allowed to become relaxed, or are forced into a state of elongation, the pain ceased; but I am now induced to believe that this is not the case, having met with the same pain, in the same degree, in parts not muscular, which was removed when the neighbouring joints were put in motion, and returned again if they were allowed to remain in one position, there being no muscles in the part affected. These attacks were unaccompanied by muscular contraction.

In the month of November, 1826, the weather being very cold, I had for several days worn woollen under-stockings. Upon going to bed, I asked the servant if the skin just above the inner ankle was red, as it was very tender to the touch, not like gout, but more threatening erysipelas. I was told it had a blush upon it, for the extent of two inches along the inner edge of the tibia. When in bed the pain was gone, but as I was dropping asleep I felt a twitter, evidently the vibratory motion of a nerve, ending in the most insupportable pain of cramp; but there was no action in the skin, nor in any muscle. Upon moving the ankle it went

off, leaving the parts very tender. It returned in a few minutes exactly in the same way; on jumping out of bed it ceased: walking did not bring it back; but lying down in the warm bed did; and in this way, every five minutes, or even at shorter periods, the pain became so similar to that of cramp, as to make it impossible for the mind to distinguish the sensations. While I moved my leg there was no return; as soon as I ceased to do so, I had an attack. Putting on a silk stocking did not prevent it. I bound the part tight with a pocket handkerchief; in a few minutes it returned. On taking off the bandage I found a ridge formed by œdema, just above where it had been applied. Worn out with repeated returns, and with vexation, the attacks became gradually more bearable. Having continued in this purgatory from eleven o'clock to four, I dropped asleep. When I got up at nine o'clock nothing was to be seen, but a slight blush on the surface of the skin over the tibia at that part, but no swelling: pulling on the stocking was in no way painful; and, as in cases of casual cramp, I had no return on using exercise, and went up and down stairs without any recurrence. A week afterwards I had a similar attack, but neither erysipelas nor gout ensued.

On the 8th of December, 1826, although wearing silk under stockings, by sitting in a draught of cold air for four hours, and then going to bed, I felt a tenderness on the inner ankle of the left leg, and its neighbourhood; this was followed by a slighter attack, but in all other respects of the same kind as those detailed, and there were slight returns next forenoon. This could be readily referred to

the nerves supplying the periosteum of the lower end of the fibula.

Till now I considered tic doloreux as an affection of the sentient extremities of the nerves of the skin, and the cramp as an affection of the fibres of a muscle, which in their contraction grasped the smaller nerves, and, by doing so, gave the insufferable pain felt in cramp; but I now consider cramp, like the tic doloreux, to be an affection of the terminal nerves in the muscles, which would produce the same pain whether the muscular fibres were contracted or elongated; and the means employed to stop the vibration in the muscular fibres also stops those of the nerves, and then the pain is removed, as was found to be the case when I moved the joints, by changing the state of tension of the nerves, but while these nerves continued in a disturbed state, the attack was liable to return.

This gives me a satisfactory explanation of the affections of the nerves of the different parts of the body, which are distinguished at present into separate complaints.

Cramp I have stated to be violent vibrations of the nerves belonging to muscles.

Life-blood, as it is termed, is too strong vibrations of the nerves of small arteries.

Tic doloreux, the too great sensibility of the terminal nerves of the surface of the skin, in which state the slightest causes throw them into violent action.

Rheumatism, preternatural vibrations in the nerves that belong to the ligaments of joints, or the tendinous parts of muscular structures.

This knowledge will assist in devising means for the

removal of the pain in these several parts, when an attack comes on, and lead us to the employment of such applications as will arrest the vibrations of the nerves affected. These are, — positive cold, in the form of ice; dry cupping; acu-punctorium, pressure, friction, and moxa.

This view of the subject will explain the relief given in the most excruciating attacks of pain from rheumatism by the use of the acu-punctorium, the effect of which is hardly felt by the patient: but the needle coming in contact with the terminations of the nerves that are in a state of vibration may put a stop to that action, although more violent applications are unable to produce such an effect.

SECT. XI.

Observations on the Nerves belonging to the Organs of Sense.

The office of the nerves belonging to the organs of sense is so different from that of the other nerves of the body, which are employed for the regulation of the actions of the arteries, absorbents, the organs of digestion, and the voluntary and involuntary muscles, that it must appear evident that they require a particular structure, and belong to a distinct and separate system, forming a communication between external bodies and the sensorium, to which all the impressions made on their terminal branches are ultimately to be conveyed, and there to be registered: while, on the other hand, the sensorium is not made conscious of the greater part of the actions going on in the animal economy; for although they are performed under the influence of nerves, many affections of these nerves do not arrive at the brain or even at the medulla spinalis.

The only nerve belonging to the organs of sense which is readily brought under our examination after death, is the optic nerve, and its terminal expansion the retina, neither of which are to be seen in the living body, since they are, while life continues, wholly transparent, as has been explained in a former volume.

The substance of this nerve, (of which, in a former description, I mentioned several of the peculiarities,) consists

of medullary substance, not indeed collected together in one mass, but made up of a great number of columns in contact with one another, and inclosed in a covering of dura mater, so that the retina is not an expansion of one mass of brain, but of so many distinct nerves in this expanded state.

This expansion of the retina has been generally believed to be necessary for the picture of the object which is to be impressed upon it, and therefore peculiar to the nerve belonging to the organ of vision.

To this opinion I confess myself to have subscribed, seeing no reason for calling it in question; but when I had contemplated at leisure the microscopical drawings of the surface of the human glans penis, made by Mr. Bauer, and compared the several clusters of the papillæ there represented, with sections of the optic nerve equally magnified, the resemblance was so great, that it led me immediately to conclude that these papillæ, which I had before considered as belonging to the secretory organs, were terminal nervous expansions, belonging to the nerves employed in receiving impressions of those pleasurable sensations, tending to excite the venereal appetite: in this I was confirmed by the number in each cluster corresponding nearly in number with the columns in the section of the optic nerve.

In pursuing this subject, the clitoris in the female was found to have clusters of similar papillæ, the use of which must be the same. To make the discovery complete, that the nerves of all the organs of sense terminate in a medullary expansion, and that this mechanism is necessary to collect a sufficiently intense perception to be conveyed

to the sensorium, I requested Mr. Bauer to examine the tip of the human tongue, under all the circumstances that could be devised. In its natural state in young subjects, after being injected with minute injection, after having the smaller arteries filled with mercury, and then immersed in rectified spirits; under all these circumstances the tongue having been examined, I must refer to the drawings themselves, and the explanations annexed to them (since verbal descriptions are but little adapted to such subjects), for the appearances it puts on; nor can I better convey an idea of the impression made on my own mind, than by saying that there was a row of medullary projections, each of which had a fine, compact, cuticular covering over it: these were convex to the external surface; and in the concavity behind were the terminations of transparent or semitransparent nerves, and an infinity of small blood-vessels.

In the tongue these terminal expansions of the nerves are convex, in the retina concave, adapting them to the form of the organ to which they belong.

From this view of the nature and structure of the nerves belonging to the organs of sense, they must, through their whole course, be distinct from the other nerves, and are adapted by their expanded termination to receive impressions from the different organs with which they are connected.

The olfactory nerves Mr. Hunter discovered to terminate in a soft substance, spread over the Schneiderian membrane.

The auditory nerve which is distinct from the portio dura, and is distinguished from all others by the name of portio mollis, is sufficient evidence that this expanded, soft

termination belongs equally to the nerves supplying all the organs of sense.

That the nerves belonging to the organs of sense arise from the brain by separate origins from the other nerves, and can be separately traced through their whole course to the organ in which they terminate, is evident in the olfactory, the optic, and the portio mollis of the auditory nerve; and it will be proved by the following cases of disease in the tongue, and the effect produced by the extirpation of part of that organ, that the nerves that communicate the sense of taste may be deprived entirely of their sensibility, while the common nerves belonging to the muscles of which the tongue is composed are in no degree injured.

The first case of diseased tongue that came under my observation, from which I was made acquainted with this important fact, was in itself very extraordinary, and gave me confidence in the treatment of other cases, that required my professional assistance. This induces me to give in detail, not only the facts that bear upon the present enquiry, but also many others of importance connected with surgery.

A gentleman, by an accident, had his tongue bitten with great violence. The immediate effect of the injury was great local pain, but it was not attended with much swelling of the tongue itself, nor any other symptom, except that the point of the tongue entirely lost its sensibility, which deprived that organ of the power of taste, so that whatever substance the person ate was equally insipid. This alarmed him very much, and induced him to state to

me the circumstances of his case, and request my opinion. I examined the tongue a fortnight after the accident. It had the natural appearance, but the tip was completely insensible, and was like a piece of board in his mouth, rendering the act of eating a very unpleasant operation. I saw him three months afterwards, and it continued in nearly the same state.

From this circumstance it appears, that the tongue itself is not particularly irritable, and that the nerves distributed upon the tip, and forming the organ of taste, are very readily deprived of their sensibility. This must arise from their being softer in texture than nerves in general, and, in that respect, resembling those belonging to the other organs of sense.

There was another circumstance in this case, which very particularly caught my attention; viz. that a bruise upon the nerves of the tongue, sufficient to deprive them of the power of communicating sensation, was productive of no inflammation or irritation in the nervous trunk, so as to induce spasms, which too commonly occur from injuries to the nerves belonging to voluntary muscles. I am therefore led to believe that the nerves supplying an organ of sense are not so liable to such effects as those that belong to the other parts of the body.

The small degree of mischief which was produced, and the readiness with which the nerves had their communication completely cut off, were to me new facts, and encouraged me, in the following case of fungous excrescence from the tongue, which at times bled so profusely as not only to endanger the patient's life, but to prevent his arriving at a

state of tolerable health, to attempt removing the part by ligature.

John Weymouth, eight years of age, was admitted into St. George's Hospital, on the 24th of December, 1800, on account of a fungous excrescence on the right side of the anterior part of the tongue, which extended nearly from the outer edge to the middle line at the tip. It appeared, from the account of his relations, that the origin of this fungus existed at his birth, and he had been a year and a half under the care of the late Mr. Cruikshank, who had removed the excrescence by ligature round its base, but when the ligature dropped off, a violent hæmorrhage took place, and the excrescence gradually returned. He then attempted to destroy it by caustic, but hæmorrhage always followed the separation of the sloughs; so that after ten trials, this mode was found ineffectual. He also removed it by the knife ten different times, but it always returned.

From this history I was led to believe, that the only mode of removing the disease was taking out the portion of the tongue upon which it grew. This was a treatment which I felt myself warranted in adopting with a view to give the patient a chance of recovery, and I was further encouraged by having found that pressure in one part of the tongue produced no bad consequences on the surrounding parts. I proceeded to remove the portion of the tongue in the following manner.

On the 28th of December, I made the boy hold out his tongue, and passed a crooked needle, armed with a double ligature, directly through its substance, immediately beyond the excrescence. The needle was brought out below,

leaving the four ligatures: two of them were tied very tight before the excrescence, the others equally so beyond it, so that a segment of the tongue was confined between each of these ligatures, in which the circulation was completely stopped. The tongue was thin in its substance, and the boy complained of little pain during the operation. Thirty drops of laudanum were given to him immediately after it, and he was put to bed. He fell asleep, continued to dose the greater part of the day, and was so easy the next day as to require no particular attention. On the fifth day from the operation, the portion of the tongue came away with the ligatures, leaving a sloughy surface which was thrown off on the eleventh day, and was succeeded by a similar slough; this separated on the fifteenth day. The excavation after this gradually filled up; and on the twentieth day it was completely cicatrized, leaving only a small fissure on that side of the tongue.

Encouraged by the result of this case, I was led to perform a similar operation upon a person at a more advanced period of life.

Margaret Dalton, forty years of age, was admitted into St. George's Hospital, on the 25th of December, 1801, on account of a tumour the size of a pea, situated on the right side of the tongue, near the edge. The history of the case was as follows. A small pimple appeared, and gradually increased without pain; the only inconvenience was, that it affected her speech, and when bruised by the teeth bled freely.

The operation was performed on the 11th of January, 1802, in exactly the same manner as has been already

described. It produced a considerable degree of salivation, which was extremely troublesome (much more so than the pain the ligatures produced), and continued till the slough came away. The ligature nearest the root of the tongue separated on the sixth day, the other on the seventh, and in three days after the separation of the second ligature the wound was completely skinned over.

A third case of this kind came under my observation, in which there was a small tumour in the substance of the tongue, about the size of a pea, which gave me the idea of its being of that kind which might terminate in cancer. The patient was a gentleman of about forty-one years of age. Upon examining the tumour, I told him of my alarm respecting its nature, and at the same time added, that I was very ready to remove it, should it be the opinion of other practitioners that such a step was advisable, — and my experience in two former cases led me to believe it might be done with safety. I therefore advised him to consult other medical practitioners of reputation, and acquaint me with their opinion. Mr. Cline was consulted, and his opinion coincided with mine, which made the patient decide upon having the tumour removed.

The operation was performed on the 28th of December, 1802. The tongue was pierced by a needle an inch beyond the tip, a little to the right of the middle line of the tongue; and the space between the two ligatures, when they were tied at the circumference of the tongue, was fully an inch. The tongue was thick, and the mass included by the ligatures was such as to make it difficult to compress it. The operation gave considerable pain, of a

numbing kind. Immediately after the operation, the part included in the ligatures became dark-coloured, particularly towards the middle line of the tongue. A salivation took place. The next day the pain and salivation were great, and the patient could not swallow, but on the day following he could take broth, negus, and other fluids.

On the sixth day from the operation the slough became loose, and the least motion of the tongue gave great pain. Upon examining the slough there was a small spot which looked red, and was surrounded by a dark surface: this was towards the right side. Upon further examination it appeared, that the part at the centre, in which the artery had its course, was not completely dead. This accounted for the red spot, as well as for the pain the patient suffered; and led me, on the seventh day, to disengage the ligature on the left (which was almost completely separated), by means of a pair of scissors, and pass another ligature through the groove to the opposite side, and tie it over the part not completely deadened. This gave great pain for a few hours, which was relieved by the use of tincture of opium. On the eight day, the patient had less pain than on the day preceding, and less salivation, and on the ninth the whole slough came away. On the thirteenth, the tongue had so much recovered itself, that there did not appear any loss of substance whatever, only a fissure of half an inch in depth, in the anterior part of it, and as that now seemed to be exactly in the centre, there was not the smallest deformity.

The preceding cases, in the view which it is intended to take of this subject, are to be considered as so many experiments, by which the structure of the tongue is, in some respects, ascertained: they enable us to draw the following conclusions.

The internal structure of the tongue is less irritable than almost any other organized part of the body; therefore the peculiar substance which is interposed between the fasciculi of its muscular fibres is not in any respect connected with the nerves which pass through its substance to the organ of taste, but is merely a soft medium, to admit of great facility of action in its different parts.

The nerves of the tongue appear to be more readily compressed, and deprived of their power of communicating sensation, than nerves in general, and any injury done to them is not productive of diseased action in the trunk of the injured nerve.

If we compare the effects of compression upon a portion of the tongue, with those of a similar compression upon the hæmorrhoidal veins when they form piles, or those of the testicle, in cases of varicose veins of the spermatic chord, which not only produce very violent local inflammation, but also a considerable degree of symptomatic fever, it is impossible not to be surprised, that the results should be so very different; since, upon a general principle, that parts are sensible in proportion to their vascularity, we have been led to conclude that the organs of sense, when inflamed, should be more exquisitely painful than any other parts of the body.

The tongue appears to have a power of throwing off its

sloughs in a shorter time than any other part. Eight or nine days is the ordinary time of a slough separating from the common parts: in the boy's tongue it was only five.

Having stated the information we derive from these cases, respecting the structure, sensibility, and irritability of the tongue, it now remains to mention the advantage to be derived from them in a professional view; and, although this is not directly in the line of physiology, it is so closely connected with humanity, that it cannot be said to be undeserving of a place in this volume.

The information derived from these cases enables us to attempt, with confidence, the removal of any part of the tongue which may have taken on a disposition to become cancerous. As this disease in the tongue always begins in a very small portion of that organ, it is, in the early stage, more within the reach of removal than when in any other part of the body; and, as the glands of the tongue are independent of each other, the cancerous disposition, by which one of them is attacked, does not so readily communicate itself to the others; and the part may be removed with a greater degree of security, against a future recurrence of the disease, than in other cases where this malady attacks a portion of a large gland, the whole of which may be contaminated long before there is any appearance of disease.

As the organs of sense were bestowed upon man, not only for the purposes of enabling him to form a sensorium within himself, that he might become a judge of the objects by which he is surrounded, and apply them to his use, it is

a curious fact, that when these nerves are not conveying information to the sensorium, respecting the objects for which they are peculiarly adapted, any friction that is applied to their terminal expansion conveys to the sensorium a pleasing gratification connected with the passions. This will occur in different degrees according to the sensibility of the individual.

That friction on the papillæ of the tongue, quite unconnected with the sense of taste, will excite the passions, and give a pleasurable sensation to the mind, is universally known by all individuals that possess strong passions. The same effect is produced by stimulating the Sneiderian membrane, as happened in the following cases: —

A young lady was attacked by an inflammation of the Sneiderian membrane of the nose, which yielded to no mode of treatment that was employed. The discharge was a mixture of a watery fluid and clots of coagulable lymph, which were occasionally detached from the surface of the membrane to which they had adhered. These, when they came away, had a very offensive smell, so much so that at last it became impossible for the patient to go into company. She was brought to London for advice, and continued under the care of an eminent physician, and equally skilful surgeon, for a year, but they were not fortunate enough, either by internal medicine or external application, to remove the dreadful stench which accompanied the discharge from the nostrils. At last, the friends, in a state of despair, gave the matter up, and were going to leave town, and bear with a malady which was considered to be incurable. Just before they left town

it was proposed that I should be consulted as a matter of form, and arrangements were made for leaving London the following morning. When I saw the patient, this candid declaration was made. Upon examining the nostrils I said that I believed I knew of an application which, in all probability, would remove the symptoms, as it had been successful in a similar case, but of a less degree of virulence; that one thing I could assure them of, none of the former practitioners consulted could have used it, since it was a composition unknown to any of the profession but Profesor Brande and myself, and if they chose to remain a fortnight longer in town, and give me an opportunity of making a trial of it, I was ready to do so, but did not choose to be interfered with during that period, and at the end of it I would candidly say whether I saw any chance of success.

To this proposal, which held out a hope, they cheerfully acquiesced. The application I employed was a supersaturated composition of the unguentum argenti nitrati, which, when first made, is not of a greater consistence than a common liniment, and even after being kept for a year does not become hard like the unguentum citrinum. The superabundant acid makes it more powerful, as well as more readily applied in its full strength. This was spread upon lint, which had been twisted round a bougie of a moderate size, curved to correspond with the course of the posterior nostrils and thus applied. When I first used it, I sat immediately before my patient, holding both her hands that she might not pull the bougies out, as well as to ascertain the degree of local pain that took place, and determine how long at one time the application should

be continued. At first there was pain, but it was not violent, and she could bear it for ten minutes, then for fifteen, and afterwards for half an hour. After using it for a few days, she made no complaint. While the ointment was used, her eyes became animated, and there was a moisture in the palms of her hands which gave an impression that she was under the influence of a pleasurable sensation. She every day got better; the discharge became less offensive, and diminished in quantity; so that some part of her expressions of pleasurable sensations might be attributed to the effect of gratitude for the relief I had afforded her. I never ventured to enquire into the nature of her feelings, but was persuaded, almost to conviction, that she received a degree of pleasure from the excitement of her passions produced by the application to the Sneiderian membrane.

I afterwards met with a similar case in a lady, who was obliged to leave India on account of it.

The application proved successful in removing the complaint; and when she was nearly well, I proposed to take my leave, and requested her to persevere in the use of the application. She said that she could not manage it, and desired me to continue my attendance. This I did till she embarked on her return to India. She then told me candidly, that the application gave her a sensation of pleasure not to be described, — and this was felt in a greater degree when applied by me than by herself.

The foregoing facts explain the gratification received from taking snuff, which makes the venders mix in it powdered glass, or some other irritating substance, to make the effect upon the membrane greater.

CHAPTER IV.

On Bone.

In the first volume of these Lectures I gave an analysis of bone, of the crustaceous coverings of sea-insects, and shell, made by my friend Mr. Hatchett. At that time it did not occur to us to continue the analysis to the bones of cartilaginous fishes or the carinæ, and scales of fishes in general.

I have been since led to this enquiry by endeavouring to ascertain the best distinguishing mark between fishes while of a small size, and when they have arrived at their full growth, the common mode of distinction by the number of rays in the fins not always proving satisfactory; and fishes at so early an age and so small a size having ova makes their breeding no test. Under these circumstances, it is with many a matter of dispute at this day whether the white-bait and sprat are distinct species, or the young of the chad and herring.

SECT. I.

On the Nature of the Carinæ, and Scales of Fishes.

That the chad is the white-bait full grown there is now no doubt, but the proof from their fins having the same number of rays is not satisfactory, since as fishes increase in size, their fins appear to vary in the number of their rays; but both the chad and white-bait having carinæ, and these carinæ being exactly of the same structure, give us a sufficient proof of their being of the same species. In what light these carinæ, peculiar to some fishes, are to be considered, is a question still unanswered. They appeared to me to belong to the skeleton, but whether in texture more nearly allied to bone than to scales, or whether scales are an approach to shell, has not, I believe, been examined. Finding this link in the beautiful series of Mr. Hatchett's investigations wanting, with a view to render the series complete, I requested Mr. Farrady to determine the nature of the component parts of the skeleton in cartilaginous fishes, and of the carinæ met with in common fishes, and to compare them with their scales and bones.

From Mr. Hatchett's analysis it was ascertained, that carbonate of lime was the hardening material of the crustacia and shells, and phosphate of lime the hardening material of bone. This gave me a direct line of distinction between bone and shell; and the object of the present

examination was to determine, whether the scales and carinæ belonged to the one or the other. From Mr. Farrady's analysis they are decidedly a species of bone.

Fishes' bone from cod's head.

Ten grains.	Carbonate of lime a faint trace only.	
	Phosphate of lime -	4.1 grains
	Insoluble animal matter dried	0.7

Fishes' scales.

Ten grains.	Carbonate of lime - - -	0.4 grains
	Phosphate of lime - -	3
	Insoluble animal matter dried	3.6

Carinæ herring.

Ten grains.	Carbonate of lime - -	0 grains
	Phosphate of lime -	2.8
	Insoluble animal matter dried	2.3

Shark's jaw.

Ten grains.	Carbonate of lime, none.	
	Phosphate of lime -	1.8 grains
	Insoluble matter dried could not determine.	

Bone, according to Brande's analysis, is as follows: —

Phosphate of lime - -	4 grains
Carbonate of lime - -	1
Insoluble animal matter dried -	5

From this investigation into the hardening materials of carinæ and scales in fishes, they may be fairly included among the parts of the skeleton, and, therefore, considered

as of sufficient permanency to assist us in the classification of the animals in which we find them.

By this means I have decided that the herring and the sprat, having their carinæ of similar form, only varying in size, are, in reality, belonging to the same species of fishes, and similar proofs are given respecting the white-bait and chad being the same fish in different stages of growth, This is beautifully illustrated by Mr. Bauer's magnified drawings that are annexed, in which the carinæ of the sprat and of the white-bait have their outline rendered distinct, and obviously corresponding exactly with those of the herring and chad, only smaller in size.

The great size to which some fishes grow, that process in many species having, probably, no other limitation than the period of their existence, is a circumstance which is not to be explained upon any principle with which we are acquainted. We do not find that it is carried into the animals that inhabit the air or the surface of the earth; it is only given to such as live in that particular medium, in which the animals can be sustained, in their progressive motion, and have sufficient supply of food.

SECT. II.

On particular Bones varying in Length in different Nations.

Little attention has been given to the varieties in size that are met with in the bones of the skeleton of the inhabitants of different countries, except, indeed, of the cranium, and that from the speculative disposition of the human mind, which has led physiologists into the indulgence of the wildest and most absurd theories.

That the skulls of the people of different nations have a characteristic difference in form, is evident to the most superficial observer; and I have no doubt, were the skeleton studied with more accuracy, many of the other bones will be found in the natives of one country, to differ materially in form from the same bones in the skeleton of the natives of other countries.

The high cheek-bones of the Highlanders in Scotland is universally known, from those bones being exposed to every one's observation. The length of the os calcis making it project backwards in the Negro, who does not wear stockings, in the West Indies, is equally conspicuous and established. The prolongation of the bones of the face in the Negro is also a character by which the natives of Ethiopia may always be distinguished.

I am led to these observations from having ascertained that the peculiar character by which the ladies in Paris

are so readily distinguished from those of London, which is universally considered to be the effect of education, and, therefore, one upon which the ladies of Paris pride themselves, and claim a superiority in the art of showing the beauties of their person, carrying themselves with more grace than the English, appears to be the consequence of the clavicle in French women being longer than in the English, which throws the scapulæ to a greater distance, and opens the chest, giving a greater breadth to that part of the person.

I was led to observe this peculiarity while at Paris, in July, 1826. In looking at the women I could not but remark, that, although they are in general shorter than the English, their average height being five feet one or two inches, they were more open-chested. This I could not satisfactorily explain; and when accompanying Baron Richerand, the head surgeon of the Hospital of St. Louis, one very hot day, to see the practice of the Hospital, I noticed, in going leisurely along one of the wards appropriated for women, whose clavicles were all exposed in consequence of the excessive heat of the weather, that they were certainly longer than usual; and I asked the Baron, whether he had known others to make the same remark. He declared he never had. This induced me to request permission to measure them, to which he readily assented. I took the exact length of seven, and found them to vary from five inches and a half to five and three quarters and six inches. After leaving the Hospital I measured the clavicles of two English ladies, and found them not to exceed five inches, although they were both

five feet four inches high. This examination, and the correspondent effect of the openness of the chest of the French women, which is so conspicuous, satisfied me that the observation respecting the greater length of their clavicles was correct; but as I was to remain two or three days longer in Paris, and might never again have an opportunity of repeating this examination, since it could only be made in the wards of an hospital, I therefore went to the Hôtel Dieu, at a time when neither the surgeons nor students were in attendance, and found the house-surgeon, with whom I was personally acquainted, at home, and ready to attend me through any of the women's wards. We measured the clavicles of nine women indiscriminately, old and young, in the order of their beds as they lay. One of them who was old, and never had been well-favoured, showed the levity almost peculiar to her country-women; she expressed not a little astonishment that she should ever have her clavicles measured. The length of the nine clavicles was six inches each, or nearly so.

I was curious to know the comparative width of the pelvis, and measured the distance between the two anterior superior spinal processes of the ossa ilii, and found it to be exactly the same in both the French and English women.

As the width of the pelvis is to give passage to the infant in the birth, it is so formed in the skeletons of people of all nations as to answer that purpose; but the use of the clavicles being less important, the length of that bone admits of being varied considerably, without being attended with any disadvantage in its ordinary uses.

I have had an opportunity of measuring the length of

the clavicles of the Negro women, and find them fully as long as in the French.

In one young Negro woman, born at Nevis, an island in the West Indies, the clavicles exceeded six inches in length. She was five feet seven inches high. In another woman of a shorter stature, the clavicles were six inches each.

From the mode in which the Negro women throw back their shoulders (in the West Indies), when walking and dancing, no one who has resided among them could doubt of their having long clavicles.

CHAPTER V.

On Muscles.

In the former volumes, the subject of muscles has been largely entered into; and although I have now resumed it, and brought to light additional facts, there still remains much to be done before the enquiry can be exhausted.

SECT. I.

On Muscular Fibres.

My principal aim was to come at the structure of a single muscular fibre; this, by perseverance and the aid of Mr. Bauer, I have been enabled to do. It was accomplished in the following manner: — A fasciculus of fibres was taken from the large straight muscle employed for occasionally raising the bullock's head. This muscle was pitched upon as its fibres run in a straight direction, and there is no fat intermixed with them, so that the fasciculi are readily separated from one another; but there was a considerable difficulty in disengaging the individual fibres, arising from the great tenacity of the gelatinous substance, by which they are held together, and warm water became necessary to make it yield. This examination is best made before life is extinct, and while the muscle retains the power of contraction, which it does in a bullock for twenty-four hours after the animal is knocked down in the slaughter-house.

The fibres consist of rows of globules, of the size of blood-globules deprived of their colouring matter, $\frac{1}{2000}$ part of an inch, and there is no doubt of these and blood-globules being the same. The jelly that keeps the globules together in the full-grown animal has less elasticity than in the calf: when a length of fibre, containing five globules,

in the calf is measured, magnified 400 times, it is exactly one inch, but when it is put on the stretch, it can be elongated to one inch and a half. In the human muscles and those of the elephant, there is no difference in the size of the globules that compose their fibres, — and the size is the same in all quadrupeds. It is necessary to make this remark from having found it otherwise in the nerves.

In the trout, the jelly, by which the globules are connected together so as to form a muscular fibre, is less elastic than in the quadruped.

When we compare these observations on the size of the globules of which a muscular fibre is composed, with those of Leuwenhoek on the same subject, we shall find strong proofs of the acuteness of his eye, and the correctness of the glasses employed in his single microscope, which he says is the best that can be made, since the glass is the smallest speck that can be seen ; and he states a muscular fibre to be composed of globules smaller than those of the red globules of the blood. Dr. Monro of Edinburgh published on this subject in 1783, making his observations chiefly in the solar microscope. He advances a theory that the fibres of muscles are the continuations of those of nerves, giving engravings to show in what manner the one fibre terminates in the other. Dr. Monro evidently had never seen a single fibre either of a muscle or a nerve, and found the smaller fasciculi of both so much alike as to mistake them for one another. Leuwenhoek and Monro, for want of a micrometer, were left to guess at relative proportions, in which they were often very unsuccessful ; but both authors, with means so very inadequate, to those

employed in the present observations, having made such near approaches to the truth, deserve very great credit; and it must be highly satisfactory to Mr. Bauer, as well as to the public, that the results of former microscopical observations so nearly approach those that are now brought forward.

Monro's theory of the nervous fibres being continuations of those of muscles is completely overset by the globules of which the two kinds of fibres are composed being of different dimensions.

Having at length brought the fibres of muscles and nerves, by means of the microscope, within the reach of our observation, we are now enabled to explain the difference between their contractile power and that of membranes and ligaments, which depends on elasticity, and is not under the direction of any part of the nervous system, while muscular contraction is wholly dependent upon it; and where no nervous control is required, muscular fibres are not employed, but elasticity is substituted for it.

These two powers of contraction are met with in the construction of animal bodies so mixed together, and the one or the other as occasion requires so increased or diminished, that when the action itself is alone enquired into, there is no mode of distinguishing the termination of the one and the commencement of the other, nor can this be ascertained in any other way than by a knowledge of the minute structure of the parts by means of microscopical observations. We thus see the fallacy of all reasoning upon any subject respecting which our information is imperfect. This was my case when I formerly denied complex

organization to be necessary in the structure of a muscular fibre, because contraction takes place in substances whose texture does not seem to admit of it; not aware that such contraction was not muscular, but depending upon elasticity acted upon by a variation of temperature of the surrounding medium and other causes.

As muscles that are constant in their action require a regular supply of blood, which can only pervade their substance while the fibres are in a state of relaxation, and this blood must be highly oxygenated, to enable it to support the continued exertion, the times at which such muscles receive their blood is deserving of notice, and has not, I believe, been attended to by physiologists.

The left ventricle of the heart, which by its contraction propels the blood received from the lungs to furnish supplies for the repairs of all the other muscles of the body, cannot receive at the same time any supply for its own fibres while they are in action; the coronary arteries, therefore, have their orifices closed during the action of the ventricle by the semilunar valves in their open state being forced against them; but when the action of the ventricle ceases, these valves shut, while the recoil of the elastic coats of the aorta forces the blood into the coronary arteries, and in the relaxed state of the muscular fibres it pervades every part of them; and as this blood comes directly from the pulmonary veins, it must contain a larger portion of oxygen than it afterwards does in any part of its circulation through the body.

SECT. II.

On the Peculiarities of the Tongue of the Xariffa, and on the Muscular Structure of Tongues in general.

The tongue is to be considered as a congeries of muscles acting upon one another, and in this respect differing from muscles applied to bones and other solid substances; but that of the Xariffa has so many peculiarities, as, in my opinion, give it a claim to be considered separately from the tongues of other animals, and viewed as a construction, in which a greater variety of actions are displayed than are to be met with in others. It not only performs the office of the organ of taste, it has besides nearly all the powers of the proboscis of the elephant, although not possessed of the same strength. They differ, indeed, in one being an elongation of the organ of smell, the other of the organ of taste. The proboscis is restrained from elongation in extent beyond one inch, by means of the cartilaginous tubes it contains; but the Xariffa's tongue, which when extended after death is seventeen inches long, can, in the living body, be so diminished in size, as to be enclosed within the animal's mouth. For this alteration in bulk some peculiar mechanism is required, since we know from experiments recorded in the Philosophical Transactions, that a muscle whether contracted or relaxed occupies exactly the same space. The chameleon, it is true,

has a power of darting the point of its tongue to the extent of twelve inches, and catching a fly at that distance: but there is a conical bone in the middle of a muscular tube, both to give direction, and by its form, when the circular fibres press upon it, making them slide forward.

In the absence of an opportunity of examining the internal structure of the Xariffa's tongue after death, I was led to the opinion that the change of size is effected by the organ containing a reservoir out of the course of the circulation, which can be filled with blood at the will of the animal, so as to give it rigidity, and enable it to extend itself for the performance of the different actions in which it is employed, with the smallest possible degree of muscular exertion.

It occurred to me, at the same time, that whatever construction may be the means by which the Xariffa's tongue is able to apply itself to such various purposes, whether that which appeared to me probable, or any other, something similar would be met with in other animals, particularly in the tongue of the deer, which after death readily admits of being drawn out to the extent of eight inches, although when immersed in rectified spirit it contracts to five inches.

For the purpose of such an examination, a deer's tongue, recently after the animal's death, was injected with minute red injection, so as to distend the arteries and show the course of the circulation in them to the greatest advantage. This tongue was afterwards divided longitudinally in a perpendicular direction, also in a horizontal one, to show the muscles of which it is composed, as well as the other parts

that it contained. From this examination the structure of the tongue of quadrupeds in general is as follows.

It is longitudinally divided into two equal portions by a middle line; the muscular structure occupies the whole of the interior substance, receiving a large supply of nerves and blood-vessels from a lateral nerve and artery that pass along the outer edge; these are imbedded in a very loose cellular tissue, the texture of which admits of the blood-vessels being distended to a very great degree, so as to enlarge the volume of the tongue; and beyond this tissue, surrounding and forming a case for the whole of the upper and lateral part of the organ, is a strong, very elastic covering of some thickness, which yields when the muscles and the trunks of the arteries are distended with blood, so as to give both extent and rigidity to the organ, and admits of the different actions in which it is employed.

There can be no doubt of the structure of the Xariffa's tongue being the same: its actions depending upon the combined powers of muscular contraction and elasticity; its increase and diminution of size arising from the blood-vessels being at one time loaded with blood, at another empty.

It is deserving of observation that these peculiarities, found in the tongue of the Xariffa for its elongation, are not extended to the camel and dromedary. These animals have a provision of another kind, enabling them to inhabit the sandy desert: this is a reservoir connected with the stomach, in which they carry a supply of water, and which is probably wanting in the Xariffa, or of a smaller size; and in lieu of it this animal has a power at all times of feeding on

plants that are alive and full of moisture, and therefore can subsist without drinking. As the sandy desert is deficient in trees, we have a proof of its not being the Xariffa's native soil, and find, that instead of the padded hoof, whose cushion is fitted for travelling in sand, it has two toes separated from each other, which are defended by a strong horny covering, enabling it to climb the higher rocky ground without stumbling. That it may have every facility in obtaining the branches and leaves of trees, its natural food, its neck is of a greater length than that of any animal of the same size, and is composed of only seven bones, exactly the same number that is met with in the human skeleton: this is evidently adapted for its reaching its food, and the smaller number of joints allows it to be kept erect at the smallest expense of muscular exertion. The tongue is every where smooth and slightly adhesive: it has spots upon it, but these are not raised above the surface. The application of this organ to the leaves before they are carried into the mouth enables the animal to reject those of noxious plants, only selecting such as have an agreeable taste. The Xariffa has been known, in several instances, when ladies have held out their hands, to apply its tongue to them, and when the hand of a man is offered, to withdraw the tongue. This may be explained upon the principle of European ladies in their dress resembling in some measure that of the Arabs who attended them, and with whom they were familiar. I am, however, disposed rather to believe, that applying the tongue to a soft smooth surface is attended with a pleasant sensation.

The tree which is said to be its favourite food is an

acacia, and now distinguished from the rest of the tribe by the trivial name acacia xariffiana. I have tasted it both boiled and in a natural state; it has a pleasant flavour, and the twigs are succulent. As the tongue in procuring and tasting its food is much exposed to the sun's rays, it is furnished with a black rete mucosum, to prevent its being blistered.

The mode in which it lays hold of the smaller succulent branches of trees, and many of its other motions, are shown in the annexed sketches, from the pencil of Mr. Agasse, of the natural size. Whether the animal in a natural state is in the habit of drinking not being known, milk is made a part of the animal's present diet: the vessel is held up breast-high; it first dips its lips in the milk, then washes its mouth, and spitting out what had been employed for that purpose begins to drink.

It chews the cud like other ruminants. At that time the body is recumbent, but the head and neck erect. It is curious to see the cud rise gradually through the length of the œsophagus to the mouth. The dung, as seen by Mr. Burchell in Africa, resembled the purl of sheep.

In comparing the quantity of moisture in common grass on which sheep feed with that of the twigs and leaves of the acacia lophantha, which nearly resembles the acacia xariffiana, it is as follows: One ounce of the leaves and twigs in drying lost three fourths: one ounce of common grass, or twenty-four scruples, lost ten; less than half by two scruples. When sheep are fed on hay, they are allowed four pounds a day and two pounds of water, which is a smaller quantity of fluid than is contained in the succulent food of the Xariffa, so that this animal cannot require any

drink for the purpose of digesting its food. While the above account was in the press, my friend Mr. Children of the British Museum showed me the feet and stomach of the lama, whose habits of life approach near to those of the Xariffa; they tend very strongly to confirm the observations I have brought forward. The lama's hoof is divided, and there are two toes with nails on their upper part, and each of them has a thick cushion or pad below.

The stomach has a portion of it, as it were, intended to resemble the reservoirs for water in the camel; but these have no depth, are only superficial cells, and have no muscular apparatus to close their mouths, and allow the solid food to pass into the fourth cavity, or truly digesting stomach, without going into these cells. A drawing of it is annexed, as forming a link between the structure of the camel's stomach and that of the bullock.

The Xariffa in the king's menagerie is a female, about two years old, is ten and a half feet high, and was taken in the Soudan district in Africa. I have adopted the name given it in Soudan, which signifies elegant or beautiful.

It was first brought to Rome in the 180th Olympiad, the 694th year of Rome, 60 years before Christ, to grace one of the triumphs of Julius Cæsar.

It was brought to London in the year 1827, to adorn the menagerie of King George the Fourth, in the seventh year of his reign.

SECT. III.

On a newly-discovered Muscle belonging to the Crystalline Lens in the Eye of the Cobytis Anableps Fish, from Surinam, to adjust the Lens for Vision at near Distances.

The cobytis anableps was known from the remarkable prominence of its eyeballs to the earliest followers of the pursuits of natural history, and it is very extraordinary that an accurate knowledge of the mechanism of the eye has never been ascertained by any anatomist even to this day, and with respect to the muscle which it is my present object to describe, no one has had the least idea of its existence.

My friend Dr. Muttlebury, while stationed at Surinam, as deputy inspector of the British troops, procured several of these fishes, and has given to me the specimens for examination; and while thus employed, I was led to the discovery of this very remarkable muscle, which is not met with in the eyes of other fishes, or any other known animal.

The first account of this eye by Artedi is very erroneous. He states that the cornea is double, the iris and aqueous humour also double, the other parts single: these assertions have been copied by all his followers without any accurate examination of their own; of this number is Blocke, Shaw, Le Cepede, and the editors of the Encyclopædia Britannica, published in Edinburgh 1824.

From these observations it appears, that there is not even now before the public an accurate account of the eye of the anableps.

In my attempt to supply this hiatus in comparative anatomy, I come forward with no small confidence, since every thing I shall state has the confirmation of Mr. Bauer's microscopical observations; and is illustrated by the exact representations from his pencil, whose accuracy is sufficiently known to the public.

The cornea is large and prominent; when it is removed, and the iris is exposed, there is an appearance of two apertures or pupils; but when the iris is more accurately examined, the loose edge that forms the pupil projects in a small degree at the upper part into the central space, and in a much greater degree on each side, so much so, that these projecting lateral portions cross the pupil and fold over one another, but do not adhere together; although this is the usual state of the iris, it admits of variation, for in some specimens these lateral projections had receded sufficiently to render the pupil of an oval form and single; it is therefore probable that the iris can so enlarge the pupil as to make it of considerable size, forming a pyramidal oval, large above and small below, while at other times it covers the middle space, and leaves only two apertures for light to come to the crystalline lens, the uppermost the largest.

That vision can only be carried on while the pupil is thus made double, there can be no doubt, since the crystalline lens instead of being spherical is not even circular, having a small projection at the lower edge, directly behind the

smaller aperture in the iris, and therefore must be adapted to make the vision through the smaller pupil distinct. When the lens is examined in the microscope inclosed in its capsule, there is distinctly seen a small bundle of muscular fibres, coming from the capsules of the vitreous humour at the lower part, and inserted into the capsule of the crystalline lens, just at the disk where the small curve joins the large one, the action of which will necessarily be, to bring the lower mammary process of the lens downwards and backwards, into the centre of the lower aperture in the iris, thus constituting a complete organ for the purpose of distinct vision at near distances, and quite independent of that part of the lens opposed to the larger aperture, which is only calculated to see more distant objects, and from the convexity of the cornea, through a rarer medium. When we consider the breadth of the body of the fish, compared with its thickness, it must lie on the water constantly upon its belly, and the prominency of the eye-balls above the head will bring the cornea upon the surface of the water, the eyeball having no lateral motion, only up and down, never moving forwards or backwards. In this respect, this peculiar formation of the iris answers a similar purpose to the marsupium in the bird, which is fixed in the posterior part of the eye, behind the crystalline lens, in a perpendicular direction, adapting the bird's vision to near and great distances; the eye-ball never moving up and down, but always either inwards towards its beak, or outwards towards the heavens. When the bird feeds, the better to see its food, the eyeball is turned towards the beak, and the moment the morsel is received

into the beak, is turned away, to be on the watch to prevent surprise.

Mr. Dollond tells me, that in the country, in watching the movements of the eyes of a tame vulture, he took his stand at a distance, unseen by the bird, and keeping his eye, while looking through a telescope, fixed upon that of the bird, he found the eyeball was in constant motion towards its beak, and from it, never moving in any other direction, as has been before explained in the former volumes. When the fish lies upon the surface of the water, and the contraction of the iris has rendered the upper aperture completely circular, by turning up the eyeball, the vision will be rendered distinct in the air over the water, as well as on its surface. When the eyeball, on the other hand, is turned down, and the smaller aperture is rendered circular by the action of the iris, all that is required to produce distinct vision from below, in the muddy water in which the fish swims, is the lens being pulled downward and backwards by this newly-discovered muscle, till it comes opposite to the middle of the smaller pupil; and the objects will be distinctly seen by being magnified; this portion of the lens, as it is adapted to near distances, producing that effect.

We thus find that, instead of the cornea, the aqueous humour, and the iris, being double, it is the lens that is double, adapting the eye to long and short vision by such a conformation, while the double pupil has the same effect that the marsupium has in the bird.

SECT. IV.

Further Observations on Progressive Motion against Gravity; and on the Surface of the Water.

We have given so many illustrations of the expense of muscular exertion being avoided by nature, whenever the necessary effect could be obtained by other means, that the subject might be considered as exhausted; this is, however, by no means the case, as has been shown in the Second Section of this Chapter, in which the presence and absence of the blood in the vessels of the tongue may be considered as a substitute for muscular action, and when on the subject of the erection of the penis, we shall find that there are reservoirs out of the course of the circulation, the filling of which alone can keep up the state of erection, without any action of the surrounding parts. Suction, which is employed to support the fly against gravity, I shall now show, performs the same office, and sustains the weight of the walrus, an inhabitant of the arctic regions, of an enormous bulk, whose proper element may be said to be the ocean, but is in the habit of sleeping and bringing forth its young upon land, and for its greater security from danger, prefers the highest and most inaccessible rocks for that purpose. This animal has its two hind-feet, called flippers, constructed exactly upon the same principle as the feet of the fly; the resem-

blance struck me from seeing one of these flippers in a very mutilated state, in maceration for preparing the bones for forming a skeleton; and when my friend, Captain Sabine, in the artillery, was about to sail with Captain Clavering to make experiments in the northern regions on the figure of the earth, I requested he would procure for me the flippers of the walrus. Assisted by Mr. Rowland, the surgeon, he complied with my request, which has enabled me to make the following observation on them.

That two animals so different in size as the walrus and fly should have any thing in common between them, particularly their feet, in the one so large as to require being diminished four diameters, to admit of the representation being brought into a quarto page, in the other so small as to require being magnified 100 times to make the parts visible to the naked eye, is in itself a very curious circumstance.

As a knowledge of the structure of the fly's foot led me to detect the use of the flipper of the walrus, so, on the other hand, an examination of the toes of this large animal brought me acquainted with the purpose to be answered by a part of the fly's foot which I did not sufficiently understand. I mean the two points called pickers by Mr. Adams, from supposing that they entered small holes in the surface on which progressive motion was carried on. This opinion I could not adopt, but was unable to find out their use, till I compared them with the outer toes in the walrus. I then saw their use was to keep down the outer edge of the web, so that the vacuum might be more suddenly and more completely established.

The hind flipper in its bones and muscles corresponds nearly to the human hand; only the lumbricales muscles are wanting, no such action as that in which they are employed ever being required.

The drawings that are annexed display the parts sufficiently, to show the manner in which the cup for producing suction is formed, and make any further verbal description unnecessary.

There is still another mode of sustaining animal bodies during progressive motion, without the aid of muscular action: this is by the common air being pent up for that purpose.

I do not mean in this place to mention the use that is made of atmospheric air permanently to diminish the weight of birds while flying, from being received into the cavities of the bones and the abdominal cells, nor the air-bags which fishes are supplied with, to enable them to rise to the surface of the water in which they are contained, as these are subjects that are generally known; but a contrivance met with in a land tortoise, an inhabitant of the Sachella islands, situate in lat. 4° south, long. 55° east, which grew to a great size in the park of Lord Egremont, where it lived for a twelvemonth, and after its death the shell was presented by his lordship to the Museum of the College of Surgeons in Lincoln's Inn Fields. It lived under ground during the cold months in a dormant state; it came out of its retreat in August, and then ate voraciously, consuming more hay than a sheep; it was in the habit of crossing a large pond in the park with great ease to itself, although it weighed 207 pounds: it died

in 1810, and I was allowed to examine it. The principal peculiarity in which it differed from other land tortoises is, that the under shell externally had a hollow cavity or basin: this is evidently given for the purpose of enabling it to swim; since, upon the animal taking the water, this cavity must remain filled with air that would be retained there, and by its buoyancy support the weight of the body.

Although this fact has been known to me ever since the year 1810, I have never heard of its being noticed by any other person. It is undoubtedly a very curious substitute for muscular action, and deserves a place among the various means devised by nature for that purpose. The depth of the cavity is four and a half inches.

The Sachella islands, from which this animal comes, abound with land tortoises and oysters: they are rocky, barren, and rise high above the water: only three of them are inhabited, — these are called Mahi, Praslin, and La Dique.

CHAPTER VI.

On the Use of the Capsulæ Renales.

Having experienced what can be effected by perseverance, and the powerful aid of Mr. Bauer's microscopical observations, in prosecuting researches into the more minute structures of animal bodies, I have been encouraged to attempt ascertaining the internal mechanism of the capsulæ renales, which has certainly not been hitherto made out, more than the use in the animal economy to which they can be applied.

I have long considered that they have a great similarity in many respects to the spleen, more especially in not having an excretory duct, and in varying greatly in size, at one time being as large again as at another. These two peculiarities distinguish them, as well as the spleen, from the glands, whether internal or external, that are met with in the body; and as the spleen is now proved to be a reservoir, in which the superabundant lymph globules are deposited, till required for replenishing the circulating blood, so the capsulæ renales form a reservoir in which some other substance is laid up in store, till wanted. The reader may smile when I mention another motive by which my mind has been actuated in taking up this enquiry.

At the age of seventeen, while assisting Mr. Hunter in the examination of the kidneys and capsulæ renales of some animal, in the dissection of which he was engaged, I asked him what was the use of these capsulæ? He answered, laughing, "I don't know: that is reserved for you to find out." This assertion of his own ignorance, from one who I thought knew every thing in anatomy, astonished me, and made me answer, that if he had not succeeded, I never could make any attempt at discovering it. So impressed was I with the difficulty that must attend such an enquiry, that I never afterwards paid the least attention to the subject, looking upon it as one beyond my reach. What terrified me at that age, and made me run away from the investigation, has, at the age of seventy-one, had a directly opposite effect, and has roused a degree of ambition to fulfil, before I die, the prophetic expression of so great a man, made more than fifty years ago.

Before I bring forward my own observations, I think it right to state how far Winslow had prosecuted this enquiry. He mentions that the capsulæ are in a more perfect state in youth than at any other period of life; in old age they are almost entirely wanting: when examined in their most perfect state they are triangular in their shape, composed of three sides and three angles: the arteries and veins when they enter the substance are inclosed in a theca as in the spleen: the arteries are very small when compared with the accompanying veins, and do not belong to one particular trunk, but are branches from the emulgents, the aorta, and vena cava, the cœliac, and diaphragmatic. There is one very large vein that occupies

the middle line of each capsula, which he calls the capsular vein; in this there are lateral orifices. He then states that there is a central cavity, which is triangular, the contents of which is an unctuous substance of the consistence of bile, varying in colour, sometimes brown, reddish, or having a yellow tinge.

Winslow's description is exceedingly accurate, until he comes to the nature of the cavity in which the contents are met with, which in reality is a branch of the large vein that has been mentioned. In justice to this great anatomist, it is right to mention, that no one since his time has been able to correct this inaccuracy, and his description of the capsulæ renales is, I believe, that which is taught in all the best anatomical lectures in Europe.

In taking up this enquiry, my first care was to procure capsulæ in their perfect state: this was attended with no small difficulty, since it is rarely the case, except in female children or young women. As soon as a perfect capsula was procured, I got Mr. Bushell, a very intelligent student in surgery, and resident house-surgeon in St. George's Hospital for the year 1827, to inject the arteries with red minute injection, and immediately to immerse it in rectified spirit, to give a degree of firmness to its substance, and prevent it from becoming putrid, which it does in a shorter period after death than most other parts of the human body.

Some of these capsulæ had the arteries alone injected, others only the veins, and in others both of these systems of vessels were filled with minute injection, the arteries with that of a red colour, the veins with yellow.

These, when they had been hardened for some time in the spirit, were dissected, and the various parts magnified in different degrees, to ascertain their structure. So rarely are the internal parts in a state fitted for accurate examination, that out of twenty not more than four showed it in a very satisfactory manner. In summer they so often ran into putrefaction before the vessels could be injected, that, during the hot months, I discontinued my investigation.

It was not till seven or eight had been examined, that their structure could be clearly made out: it was then ascertained that there is no real triangular cavity, but three large venal branches anastomosing freely with one another, and having lateral openings through their coats communicating with the parenchymatous substance immediately behind the veins. These branches were loaded with a continued mass of transparent oil in a fluid state, and when it was removed from the cavity of the vein and placed in the field of the microscope, it was found to be perfectly pure. The appearance of the parts after the oil had been removed is represented in the annexed drawing.

Having ascertained these two important facts; one, that the contents of the capsulæ renales is a pure liquid oil, the other that it is contained in the large capsular vein and its neighbouring anastomosing branches, which appear to have their capacity increased for this particular purpose, I considered the lateral apertures which open into the parenchymatous substance behind the veins intended to admit of the oil being squeezed out of the vein and taken up by the absorbent vessels, and that no further examination

was necessary to explain the use of the capsulæ renales, as it must be a filter by which any oil that is left in the arterial branches that are near the kidneys may be separated and prevented from making its escape by the tubæ uriniferæ of these glands. That no oil does enter into the urine has been ascertained by Professor Brande's analysis of that secretion, which he made at my request to ascertain whether alcohol was ever present in it.

Oil I have already considered in the former volumes to be formed in the colon; and to be so important an article of nutriment to the body, that the greatest care is taken in the actions of the animal economy to economise its expenditure, — and there is no other natural outlet for it, from the circulation, than by the terminal arteries.

That it is an important ingredient in the semen is sufficiently proved by the single fact, that the buck, in the fallow-deer tribe, is abundantly supplied, even loaded, with fat at the commencement of the breeding season, and at the end of the rut there is not a particle left on any part of its body. In early life it is laid up in the loins in the neighbourhood of the kidneys; and it is only in young people that the capsulæ renales appear to be employed to prevent its escape, beyond that period the quantity being too small at any one time, in the circulation, to call for such a filter.

Those who are employed in fattening cattle for sale are so well acquainted with the particular parts in which the fat is deposited, that they can tell by experience the weight of a sheep by only feeling its loins; and are seldom wrong more than a pound in the conclusion formed from such an examination.

The readiness with which animal substance after death is converted into adipocere, which has been already explained, naturally leads to the belief, that, by an equally simple process, fat may be reconverted into animal substance.

CHAPTER VII.

On the Coverings of the Bodies of Animals, consisting of Cuticle and Rete Mucosum.

SECT. I.

On the Cuticle in a healthy State, and produced by Disease.

The cuticle is the most external of these coverings: it is composed of the albumen of the chemists, which is the coagulable lymph that has been explained in these volumes, forming one of the ingredients of the circultaing blood, and exuded from the terminal arteries of the cutis.

When separated from the surface on which it lies, it has the appearance of an extremely thin membrane, devoid of colour, and extremely vascular. It is met with in various other forms besides that of a membrane; and these have names derived from the uses to which they are applied, and the appearances they put on, to distinguish them from one another. These are hair, nail, hoof, horn, and

feather. They not only serve as a covering to protect the terminal nerves of the cutis from injury, but to prevent the escape of the animal's heat, and serve as weapons, by which the animal can not only defend itself, but can attack its enemies.

The shape or configuration of these very different cuticular productions is determined by the surface on which each of them is formed ; and there is a mould on the cutis to give the nail and the hoof its precise thickness, and all the necessary indentations and irregularities: the part first formed is pushed forward, but does not afterwards undergo any change, unless it is tending to decay, when the fibres separate from one another.

The horns of one kind are formed upon a core, as in the black cattle; but the horn of the rhinoceros is wholly cuticular.

These may be called the natural cuticular coverings, which are renewed in the same way when accidentally destroyed. For the mode in which the cutis and the cuticle which is spread over its surface are formed, I must refer to the former volumes of this work. The very important discovery of the evolvement of the carbonic gas from the blood, forming the vessels in the coagulum which it pervaded, was completed in 1817, and was no sooner proved to take place in the living body than the source of the blood-vessels in the cutis, and the lymph-vessels in the cuticle, was completely developed, and to the plates upon that subject the reader must be referred.

In this reference he will find, that the surface of the cutis is nothing more than a rete mirabile of blood-vessels

forming the surface, and immediately over it there is a similar rete mirabile of vessels carrying lymph. Blood globules are circulated in the one received from the blood in the neighbouring blood-vessels; lymph globules are received into the other. The readiness with which the layers of cuticle succeed one another is very extraordinary, and very naturally excited the admiration of Winslow; but when the drawing in a former volume is consulted, in which there was a succession of cuticles in less than twenty-four hours, and each of them vascular, although the vessels did not carry red blood, this admiration will cease to strike the reader, from seeing the simplicity of the process by which this rapidity was produced.

Having made this reference to the third volume of the present work, I shall not say more of the cuticle in this place, reserving any new observation till I have shown the nature of the rete mucosum with which it is in immediate contact and intimately connected, so much so, that they cannot well be satisfactorily demonstrated unless they are taken together.

Besides the natural cuticle, there is what may be called an unnatural formation of cuticle, in consequence of the structure of the skin having undergone changes from disease; and when such formation takes place, it has been considered as a monstrous production.

In giving the history of this deviation from the natural actions of the body, rare in its occurrence, and so remarkable in the formation it produces as almost to exceed belief, it will be thought right to bring proofs of such things really taking place. This I have no difficulty in

doing, since there are two women now alive in England, labouring under a disease of this kind, one of which has come within my own immediate observation, and we have upon record the testimony of many respectable authors, who have had opportunities of examining such productions.

The two following cases contain so very accurate and distinct a history of the growth of these horns through their different stages, as to make more minute details entirely unnecessary.

Mrs. Lonsdale, a woman fifty-six years old, a native of Horncastle in Lincolnshire, fourteen years ago observed a moveable tumour on the left side of her head, about two inches above the upper arch of the left ear, which gradually increased in the course of four or five years to the size of a pullet's egg: when it burst, it for a week continued to discharge a thick, gritty fluid. In the centre of the tumour, after the fluid was removed, she perceived a small soft substance, of the size of a pea, and of a reddish colour on the top, which at that time she took for proud flesh. It gradually increased in length and thickness, and continued pliable for about three months; it then first began to put on a horny appearance. In two years and three months from its first formation, she became desperate by the increased violence of the pain; she attempted to tear it from her head, and with much difficulty, and many efforts, at length broke it in the middle, and afterwards tore off the root, leaving a considerable depression which still remains in the part where the horn grew. Its length altogether is about five inches, and its circumference at the two ends about one inch, but in the middle

rather less. It is *curled* like a ram's horn contorted, and in colour much resembling isinglass.

From the lower edge of the depression another horn is now growing, of the same colour with the former, in length about three inches, and nearly the thickness of a small goose quill; it is less contorted, and lies close upon the head.

A third horn, situated about the upper part of the lambdoidal suture, is much curved, above an inch in length, and more in circumference at its root: its direction is backwards, with some elevation from the head. At this place two or three successive horns have been produced, which she has constantly torn away; but as fresh ones have speedily followed, she leaves the present one unmolested, in hopes of its dropping off.

Besides these horny excrescences, there are two tumours, each the size of a large cockle, one upon the upper part, the other about the middle of the left side of the head: both of them admit of considerable motion, and seem to contain fluids of unequal consistence; the upper one affording an obscure, the other a very evident fluctuation.

The four horns were all preceded by the same kind of encysted tumours, and the fluid in all of them was gritty: the openings from which the matter issued were very small: the cysts collapsed and dried up, leaving the substance from which the horn proceeded distinguishable at the bottom. These cysts gave little pain till the horns began to shoot, and then the pain became very distressing, and continued with short intervals till they were removed. This case is drawn up by the surgeon who attended the

woman for many years, which gave him frequent opportunities of observing the growth in its different stages, and of acquiring an accurate history of the case.

Mrs. Allen, a middle-aged woman, resident in Leicestershire, had an encysted tumour upon her head, immediately under the scalp, very moveable, and evidently containing a fluid. It gave no pain unless pressed upon, and grew to the size of a small hen's egg. A few years ago it burst, and discharged a fluid; this diminished in quantity, and in a short time a horny excrescence, similar to those above mentioned, grew out from the orifice, which has continued to increase in size; and in the month of November, 1790, the time I saw her, it was about five inches long, and a little more than an inch in circumference at its base. It was a good deal contracted, and the surface very irregular, having a laminated appearance. It moved readily with the scalp, and seemed to give no pain upon motion; but when much handled, the surrounding skin became inflamed. This woman came to London, and exhibited herself as a show for money; and it is highly probable, that so rare a sight would have sufficiently excited the public attention to have made it answer her expectations in point of emolument, had not the circumstance been made known to her neighbours in the country, who were dissatisfied with the measure, and by their importunity obliged her husband to take her back immediately into the country.

There are many cases upon record of similar horns having sprung up from the surface of the scalp, and other parts of the human body; but these have hitherto been considered as lusus naturæ, only to be wondered at, and not

in any way to be accounted for; and the vague and indefinite terms in which authors write upon this subject show plainly that they were ignorant of the cause of this appearance, and their accounts of it are not very satisfactory to their readers.

In the *Ephemerides Academiæ Naturæ Curiosorum*, there are two cases of horns growing from the human body. One of these instances was a German woman *, who had several swellings, or ganglions, upon different parts of her head, from one of which a horn grew. The other was a nobleman †, who had a small tumour, about the size of a nut, growing upon the parts covering the last two or lowermost vertebræ of the back. It continued for ten years, without undergoing any apparent change; but afterwards enlarged in size, and a horny excrescence grew out from it.

In the History of the Royal Society of Medicine ‡, there is an account of a woman, ninety-seven years old, who had several tumours on her head, which had been fourteen years in growing to the state they were in at that time; she had also a horn which had originated from a similar tumour. The horn was very moveable, being attached to the scalp, without any adhesion to the skull. It was sawn off, but grew again; and although the operation was repeated several times, the horn always returned.

Bartholine, in his Epistles §, takes notice of a woman

* Ephem. Acad. Nat. Cur. Dec. iii. An. 5. Append. p. 148.

† Ibid., Dec. i. An. 1. Observat. 30.

‡ Histoire de la Société Royale de Médecine, 1776, p. 316.

§ Epistol. Thom. Barthol.

who had a tumour upon the scalp, covering the temporal muscle. This gradually enlarged, and a horn grew from it, which had become twelve inches long in the year 1646, the time he saw it. He gives us a representation of it, which bears a very accurate resemblance to that which I have mentioned to have seen in November 1790. No tumour or swelling is expressed in the figure; but the horn is coming directly out from the surface of the skin.

In the Natural History of Cheshire *, a woman is mentioned to have lived in the year 1668, who had a tumour or wen upon her head for thirty-two years, which afterwards enlarged, and two horns grew out of it: she was then seventy-two years old.

There is a horny excrescence in the British Museum, which is eleven inches long, and two inches and a half in circumference at the base, or thickest part. The following account of this horn, I have been favoured with by Dr. Gray, taken from the records of the Museum. A woman, named French, who lived near Tenterden, had a tumour or wen upon her head, which increased to the size of a walnut; and in the forty-eighth year of her age this horn began to grow, and in four years arrived at its present size.†

* Lee's Natural History of Lancashire and Cheshire.

† The following extract is taken from the Minutes of the Royal Society, Feb. 14. 1704–5: —

" A letter was read from Dr. Chariere, at Barnstaple, concerning a horn, seven inches long, cut off the second vertebra of the neck of a woman in the neighbourhood.

" Dr. Gregory said, that one of seven inches long, and of a dark-brown colour, was cut off from a woman's temple in Edinburgh.

" Dr. Norris said, that two horns had been cut off from a woman's head in Cheshire."

There are many similar histories of these horny excrescences in the authors I have quoted, and in several others; but those mentioned above are the most accurate and particular with respect to their growth, and in all of them we find the origin was from a tumour, as in the two cases I have related; and although the nature of the tumour is not particularly mentioned, there can be no doubt of its being of the encysted kind, since, in its progress, it exactly resembled those, which, after having remained stationary for a long time, came forwards to the skin; and the horn being much smaller than the tumour preceding the formation of the horn, is a proof that the tumour must have burst, and discharged its contents.

From the foregoing account it must appear evident, that these horny excrescences are not to be ranked among the appearances commonly called *lusus naturæ;* nor are they, altogether, the product of disease, although undoubtedly the consequence of a local disease having previously existed; they are, more properly speaking, the result of the new cutis formed by the lining of the cyst being unable to produce the proper materials to cover it with cuticle. This species of excrescence is formed as a substitute for the natural cuticular covering.

To explain the manner in which these horns are formed, it will be necessary to consider the nature of encysted tumours a little more fully; and in doing so, we shall find that this particular species does not differ in its principle, nor materially in its effects, from many others which are not uncommonly met with in the human body, as well

as in those of many other animals, which, as they are more frequent in their occurrence, are also much better understood.

Incysted tumours differ exceedingly among themselves, both in the nature of their contents, and in their progress towards the external surface of the body. Many of them have no reference to our present investigation; it is only the more indolent kind to which I mean to advert: some of these, when examined, are not found to contain a fluid, but a small quantity of thick, curd-like matter, mixed with cuticle broken down into small parts, and upon exposing the internal surface of the cyst, it is found to have an uniform cuticular covering adhering to it, similar to that belonging to the cutis on the surface of the body, from which it only differs in being thinner, and more delicate, bearing a greater resemblance to that which covers the lips. Others of this kind, instead of having cuticle among their contents, are filled with hair, mixed with a curdled substance, or hair without any admixture whatever, and have a similar kind of hair growing on their internal surface, which is cuticular; so that it appears to be the lining of the cyst that becomes similar to the cutis, and, notwithstanding the circumstances in which it is placed, forms a cuticular covering with hair. In what way this change is brought about is not easily determined; but from the indolence of these complaints, it most probably requires a considerable length of time to produce it. That the lining of the cyst really does possess powers similar to cutis, is proved by the circumstance of a succes-

sion of cuticles thrown off from the surface being found in the cavity of the cyst. It has a similar power respecting the formation of hair, and sometimes the cavity is filled with it, so great a quantity has been shed by the internal surface. Besides these circumstances, the hair found in the cyst corresponds in appearance with that which grows upon the body of the animal, and when incysted tumours of this kind form in sheep, they contain wool. What is still more curious, when such cysts are laid open, the internal surface undergoes no change from exposure, the cut edges cicatrize, and the bottom of the bag remains ever after an external surface. Different specimens, illustrative of the above-mentioned circumstances, are preserved in Mr. Hunter's collection of diseased parts.

The cysts that produce horny excrescences are very improperly considered as giving rise to horns; for if we examine the mode in which this substance grows, we shall find it the same with the human nails, coming directly out from the surface of the cutis. It differs from the nails, in not being set upon the skin by a thin edge, but by a surface of some breadth, with a hollow in the middle, exactly in the same manner as the horn of the rhinoceros *; at least this is evidently the case in the specimen preserved in the British Museum, and in one which grew out from the tip of a sheep's ear; they are also solid, or nearly so, in their substance.

* The horn of the rhinoceros is a cuticular appendage to the skin, similar to nails, and other cuticular excrescences, being in no respect allied to horns but in the external appearance.

This mode of growth is very different from that of horns, which are all formed upon a core, either of bone or soft parts, by which means they have a cavity in them.

Encysted tumours in different animals would appear, from these observations, to be confined in their production to the cuticular substance proper to the animal in which they take place; for, although cuticle, hair, nail, hoof, and horn, are equally productions of animal substance, only differing in trivial circumstances from each other, we do not find in the human subject any instance of an encysted tumour containing a substance different from the cuticle, hair, and nails of the human body, to which last these horny excrescences are closely allied, both in growth, structure, and external appearance; and, when of some length, they are found to be so brittle as to break in two upon being roughly handled, which could not happen either to hoof or horn. In the sheep they produce wool instead of hair; and in one instance in that animal, where they gave rise to an horny excrescence, it was less compact in its texture, and less brittle, than similar appearances in the human subject; upon being divided longitudinally, the cut surface had more the appearance of hoof, and was more varied in its colour than nail.

Encysted tumours being capable of producing horns, upon the principle we have laid down, is contrary to the usual operations of nature, for horns are not a production from the cutis; and although not always formed upon a bony core, but frequently upon a soft pulp, that substance differs from common cutis in its appearance, and extends

a considerable way into the horn: it is probable that this pulp requires a particular process for its formation.*

* A sheep, about four years old, had a large horn, three feet long, growing upon its flank. It had no connection with bone, and appeared to be only attached to the external skin. It dropped off in consequence of its weight having produced ulceration in the soft parts to which it adhered. Upon examining it, there was a fleshy substance, seven inches long, of a fibrous texture, filling up its cavity upon which the horn had been formed.

SECT. II.

On the Rete-Mucosum.

In a former part of this work, I endeavoured to show that the rete-mucosum, in the Negro, answers the purpose of preventing the cutis from being scorched by the sun's rays, the black colour decomposing them upon their arrival at the surface of the body. I shall now consider this mucous membrane more particularly, both with respect to its nature and formation.

The rete-mucosum differs from the cuticle in being composed of the gelatine of the chemists; it readily dissolves in hot water, and when it cools, if sufficiently concentrated, it becomes of the consistence of jelly, and is then readily soluble in cold water. It is a production from the cutis, which is itself, in part, made up of gelatine.

The following observations on the rete-mucosum are principally the result of the appearances it puts on when examined in the microscope, and, therefore, I am indebted to Mr. Bauer for any new light that is thrown by them on this part of minute anatomy.

It is a kind of pigment laid upon the posterior surface of the cuticle, between it and the cutis, and is imbedded in the interstices of the vessels which compose the base of the cuticular covering, and form aureolæ round the hairs of the skin; this pigment is in greatest quantity behind

these aureolæ, and is, in every respect, similar to the nigrum pigmentum of the eye; an appearance more readily understood from having no cuticular covering, and is of so soft a texture as in no way to be separable from the choroid coat on which it lies, but by washing or rubbing.

The pigment that composes the rete-mucosum appears in the microscope to be lodged in small cups or cavities, but these are only the aureolæ of blood-vessels that surround the hairs.

This pigment is of different colours both in the eye and on the skin, and when this is the case the colours of the two correspond. In the turkey's leg it is red, in the mackarel it is the colour of silver, but when examined in the microscope this appears to be a black powder. It is a circumstance deserving of notice, that the rete-mucosum is not formed in the fœtus in utero, although the surface of the body is completely covered with cuticle during the earliest period of utero-gestation.

The nigrum pigmentum of the eye appears to be subject to the same laws respecting its production, not being met with till after birth.

From what this arises has not hitherto been explained; but that there is a principle in the animal economy, on which it depends, is proved by the mode which nature has employed for repairing the coverings of the body, when they are removed by accidental violence. In injuries to the skin the cuticle is first renewed, and long afterwards the rete-mucosum; and, under many circumstances, this last is never reproduced.

Why the rete-mucosum is not formed while the fœtus is

in utero, may be explained by the circumstance that it can be of no use while the young is retained there, nor for some time after the birth, while the cuticle is necessary to defend the cutis in the different movements of the limbs of the fœtus in utero.

Light is known to be necessary for plants to acquire colour, since those that are kept in the dark are always white. It may be the same with animals; and the rete-mucosum that gives colour to the skin and hair, may, also, require light for its formation, or, at least, for its being rendered visible.

It is a circumstance favourable to this opinion, that the child of a black woman is born without any difference of colour from the child of a white woman, and it is nine days before it becomes quite black. Puppies are born without any nigrum pigmentum behind the retina, and it requires nine days before their eyes can bear the light.

In a still-born child there is no appearance of nigrum pigmentum in the eyes; the inner surface of the choroid coat is rough, and has a brown tint; and, just behind the ciliary processes, there is a dark line. I examined the eye of a child who died in the birth, and found this to be the case.

The hair, whose root is in the cutis, is a cuticular tube originating from a pulp; it has a lining of rete-mucosum from which it receives its colour. This is either later in its formation than the hair itself, or later in receiving its colour, as the rete-mucosum is not vascular. This is easily explained, since its future changes may depend upon the absence of light, or the substance being decomposed.

One of these changes is, the hair becoming grey, an effect of the rete-mucosum separating from the cuticle over it, and in the act of being decomposed losing its bulk, and remaining in this shrivelled state within the cuticular tube, which is deprived of the colour, whatever it was, that before distinguished it. This fact is distinctly seen when the hair is examined in the microscope.

These facts respecting the rete-mucosum are, I believe, quite new, nor was it known that the hair had a lining giving it colour: they could not be discovered till the microscope had been brought to its present state of perfection, and the management of highly magnifying glasses was well understood.

This explains the hair that is concealed from the light by the clothes being always lighter than that on the head, also the hair on wild animals in all warm climates being black on the back, and lighter on the belly.

From being ignorant of the real texture of the cutis and cuticle, and of the readiness with which the laminæ of cuticle can be separated from one another, it is common with anatomists to separate the external laminæ of the cuticle by heat, and then raise, as a distinct membrane, the lamina next the cutis with the rete-mucosum spread upon it, and demonstrate this last as the rete-mucosum, which is incorrect, since it is, in fact, a cuticular lamina, with the pigment spread upon it.

This structure of hair makes it no longer difficult to explain the sudden change of its colour, which has been stated to take place upon any sudden emotion of grief or anxiety, and shows its connection with the terminal nerves

of the cutis, by whose influence it can be suddenly destroyed or removed.

As the rete-mucosum, whether it lines the cuticle upon the surface of the body, the tubular cavity of the cuticle that forms hair, or forms the surface on which the retina is spread, appears to be acted on by the same circumstances, so that when the hair becomes grey, the skin loses its colour, and the person can only see with a weak light.

The truth of this remark was put to the test on one of the in-pensioners of Chelsea Hospital, 106 years of age. His hair turned grey when he was seventy-six, and after eighty-six he had been unable to see in strong lights, and very badly in any light.

Baron Larrey, when in London, told me, that while he was at Brest, in the year 1788, he saw Louis Bourbon, who had been shut up in a subterraneous prison for thirty-three years, secluded entirely from the light. He was then sixty-seven years old: during the day he was completely blind, and only saw objects in the dark: his hair was absolutely blanched: when his hair first became white, the pigment of his eyes had undergone the same change, which gives a satisfactory explanation of this curious phenomenon.

The shedding of the hair and feathers in the arctic regions, during the six months in which they are not visited by the sun, produces an effect of the same kind: the fact is notorious; and at that season, the nigrum pigmentum being absent, fits the animals and birds to see with the weak light afforded them.

When a black hair and a grey one are laid across one

another in the field of the microscope, and examined with a lens magnifying two hundred times, the tubular form of the grey hair is distinctly seen, and the colouring matter forms a line of black in the middle of the tube, while the black hair has the rete-mucosum in close contact with the cuticle.

That there is no such thing as colour but what is derived from the solar rays, is an opinion which receives strong support from the observations that have been made, and from examining coloured surfaces with the greatly improved microscopes of the present day, in which it is shown that simple lines closely arranged is all that is necessary to reflect the rays of light in their most brilliant colour.

It is many years since, that I proved the permanency of colour from reflected light, by taking an impression of a mother-of-pearl button on sealing-wax, which had all the colours.

My friend Mr. Barton of the Mint, who possesses a dividing machine, handed down to him from his father-in-law, that ingenious artist Mr. Bird, at my request several years since, when I was engaged on this subject, divided a piece of steel by lines of 10,000 parts of an inch, and crossed these by the same number of lines. When this surface was exposed to the sun's rays, it emitted the rays of light in the most brilliant colours; when seen in the field of the microscope, illuminated by an Argand's lamp, these colours were still brighter.

The wings of the diamond-beetle, whose colours are so vivid during sunshine or strong light, when placed in the field of one of the new acromatic microscopes of Dollond, represent a number of lines of the greatest degree of

minuteness, the arrangement of which produces the colours; and it is now the test of the magnifying power of a microscope, its being able to show these lines with a sufficient degree of accuracy. It is a similar arrangement of fibres from which the feathers of tropical birds receive the gaudy colours for which their plumage is so much prized: they must, however, be magnified 400 or at least 200 diameters, to render them visible to the eye, and this only in one particular direction.

A French gentleman has just discovered a substance by means of which he can so prepare any polished surface of silver or tin, that the sun's rays reflected from any object will be so fixed as to leave its image. The discovery he considers not brought to perfection, and therefore has not promulgated it: he has presented me with a specimen of this art, which will prove a very valuable discovery, since the outline of the representation must be perfectly accurate, however much it is diminished.

CHAPTER VIII.

On the Propagation of the Species in the different Classes of the Animal Kingdom.

The formation of living animals is one of the most wonderful effects of almighty power, and one that is beyond the comprehension of the limited intellect bestowed upon mankind; but the modes adopted by the Creator for propagating from the parent stock each individual species is not beyond the reach of human examination.

This, however, cannot be accomplished without much ingenuity of contrivance in aid of the powers that naturally belong to our organs of sense, since so small is the first germ from which an offspring is hereafter to be produced, that unless it is magnified 400 times, it does not become visible to the human eye.

It will be seen in a former volume, that I was the first anatomist who had the good fortune to detect the germ from which man is produced,—and led on by that good fortune, I have gone into considerable extent in this investigation, and have left for those who follow me very little to make out in the mode of propagation of uterine animals furnished with or without a placenta.

As I originally set out in this enquiry with the formation of this germ in the highest order of animals now in existence, the human species and quadrupeds, and as I have demonstrated that in all of them an ovum is first formed, I shall now renew my enquiries by beginning with the propagation of the species in the most simple animals in nature, and shall, in the present volume, prove to the satisfaction of my readers, that all the animals belonging to the animal kingdom are produced from an egg.

The power which many animals possess of renewing themselves by the growth of such parts as have been cut off, as the polypi, is foreign to the present enquiry.

The sea-worms, with which I shall begin my present researches, are unquestionably the most simple in their organization of all the animals that can be subjected to dissection by the anatomist; and there are anatomists who deny that in them an egg has ever been detected as the source from which the animals originate: one individual, after having had the subject for twenty years under consideration, at the end of that period has ventured to publish this opinion to the world.

SECT. I.

On the Propagation of the Species in the Oyster.

That many animals have in the same individual both the male and female organs, and impregnate their own ova, has been already mentioned and illustrated in the teredines and the different kinds of eel.

The oyster and fresh-water muscle are of this kind: in the oyster, which has no organs fitted for loco-motion, and is consequently confined to the same spot, its propagation could not be otherwise carried on; but this argument does not apply to the fresh-water muscle, which has a foot, and is capable of moving from one place to another.

Mr. Bauer and myself were engaged for five seasons in examining the generative organs of the oyster, before we were satisfied respecting the mode in which the ova left the ovarium, and passed out through the oviduct. In the year 1826 we completed this investigation, and illustrated the appearances that were observed by magnified drawings of the different parts; and the oyster, we have already asserted, is probably the most simple in its structure of all the animals that come under the knife of the anatomist as the subject of dissection.

We have already stated, that the substance which performs the office of brain and spinal marrow is even smaller in them than in the fresh-water muscle. There

being no organs for loco-motion in the oyster, it cannot go in search of food, but is reduced to the necessity of taking whatever is brought to it by the fluid that surrounds it. The only expenditure of muscular action that we know of, except the peristaltic motion of the intestinal canal, and the action of the heart, is the contraction of the great muscle by which the two shells are closed, which is only occasionally brought into action to exclude attacks from its enemies. No exertions of the oyster itself are required to keep the shells open; that is done by an elastic ligament at the hinge, which, the moment the muscle that kept the shells shut relaxes, by its elasticity makes them fly open.

The ova are so exceedingly small, that it is very difficult to discover the situation of the ovarium; and this is only to be done, in the first instance, in the breeding season, and even then requires the aid of the microscope.

In this country, where the oyster-beds are not allowed to be disturbed during the breeding seasons, we labour under considerable disadvantage; and I am indebted to my friend, Mr. Copland Hutchinson, who afforded me an opportunity of examining some oysters taken from a private bed near Sheerness, once a week, during the whole time they are not brought to market for sale.

In France, where there are no such restrictions respecting the oyster-beds, they are sold in Paris throughout the four months in the names of which there is no *r* equally as in all the others; and as fishes are considered to be in season while breeding, and after they have spawned to have no flavour, for it would be an affront to give an epicure a shotten herring, I took advantage of the

opportunity which offered itself from being in France in July, 1826, to taste the oysters, but cannot consider them good at that period: this may arise from the ova having previously escaped from the ovarium, and therefore the oysters are then under similar circumstances with fishes that have spawned.

The ovarium is not evanescent as soon as the breeding season is over, although to the naked eye it would appear so; for new ova very soon begin to form in it. The ovaria may be considered double, as in fishes having the liver separating them; one lying immediately under the membrane that lines the convex shell, the other immediately over the membrane covering the flat shell: the ova hang on pedicles attached to the membrane of the ovarium, and this membrane becomes thicker and more conspicuous as they increase in size.

They are large enough to assume a distinct form in the month of March. When they have arrived at their full size, a white liquor of the consistence of cream is met with, in which they appear to float, probably secreted for their fecundation.

This happens towards the end of June, and a tube has now become visible, although not before distinctly seen. It opens by one end in the edge between the two ovaria, and the orifice communicates with both of them. At this time the ova drop from their pedicles, and therefore have arrived at their full growth; and there can be no doubt of their impregnation having taken place in the ovaria, for those that are detected passing along this tube, which is the oviduct, have already acquired a shell, and each ovum

is enclosed in a vesicle, so that the size of the ovum can no longer be ascertained, but the cell met with in the pearl, as I shall explain, must be an exact cast of it, when it has completed its growth.

The oviduct forms a sheath in which the intestine is enclosed, and terminates in an orifice between the lips at the mouth of the intestine, from which the young oysters pass out mixed with a purple mucus intended for their nourishment. From this situation the opening and shutting of the shells of the parent-oyster must throw out the young along with the salt-water.

In this stage of their growth, they are often a prey to small gelatinous sea-worms, which get within the shells of the parent-oyster, and gorge themselves with the young, their stomach being found quite distended with them.

The parts which I have endeavoured to describe will be much better and more clearly comprehended by inspecting the annexed drawings; I shall therefore not go into a more minute detail.

The structure of the liver is so analogous to that of the ovaria, when they contain ova, that, were not the colours different, they could not be distinguished from one another. This will be readily seen in the drawings.

In the oyster there appears to be no connection between the oviduct and the branchiæ; but the membranes of the branchiæ, and the fringed edges of the mantle by which they are enclosed, are much thicker and more vascular during the breeding season; and many peculiar appearances, upon the edge of these enveloping membranes, are much more conspicuous during that period. For this reason, although I am by

no means acquainted with their use, Mr. Bauer has given a faithful representation of their appearance; from which every one may draw his own conclusions.

The stomach and course of the intestinal canal has on a former occasion been explained.

The heart has two auricles as in the teredines; and the vessels from the ventricle supply the great muscle for closing the shells, in the same manner as in the teredines, there is a large artery goes to the muscles of the boring shells.

SECT. II.

On the Mode of propagating the Species in the Fresh-Water Muscle.

In this animal the ovaria have the same situation and appearance as in the oyster. The ova are of the same size; they are impregnated in the same manner before they are expelled from the ovaria, and while attached by their pedicles have the same resemblance to the structure of the liver, and when abortive go through the same process and become pearls.

The ova first appear so as to be distinctly seen in the ovaria in the field of the microscope about the 10th of August.

They are first detected passing into the oviducts about the 20th, and in about the 12th of September they generally have all arrived within the numerous canals of which the oviducts are composed, situated in the middle space between the laminæ of the membranes composing the gills or branchiæ.

That they have been impregnated before they leave the ovarium is evident from the change they have undergone: each ovum is enveloped in a membrane containing a liquid; and the rudiments of the membranes, which are to be afterwards coated with cretaceous matter and nacron, are constantly opening and shutting for the acration and

nourishment of the embryo. While in this situation many of them were seen turning round as it were upon a centre. This circular motion was taken notice of by Leuwenhoek, who was so much struck, that he called his wife and daughter to witness it, that there might be sufficient evidence of its existence. It is a circumstance deserving of notice that when Mr. Bauer first saw it, not then knowing it had been before observed, he called in a young female servant, and told her to look through the microscope and tell him what she saw; she immediately answered, There is a little white thing turning round and round. This revolving motion of the embryo, as it appeared to be, attracted our particular notice, and from its resemblance to that motion which is employed to give the form to the lump of clay in manufacturing plates, and cups and saucers, I indulged in the notion that it was to serve this purpose in forming the shells; but Mr. Bauer's eye could not be long deceived, or prevented from coming at the true cause of such motion, and at last he discovered that a small worm had got into the membranous bag, and while it was feeding upon the embryo, was moving in this circular manner, and carrying the young muscle with its imperfectly formed shells along with it, being itself entirely concealed from the eye of the observer.

The young remain incarcerated in the cells of these very unusually formed oviducts, which, if like any thing, resemble the cells of a bee-hive, but only in the use and subdivision; keeping the young in security, and separate from each other, till they have arrived at their full growth,

and are fit to provide for themselves. They do not leave their cells till October and November.

The young muscles are much longer retained in the oviduct than the oysters: this may arise from the first living in fresh-water, which cannot be considered to give the same supply of nutriment with salt-water; and its structure being more complex, longer time will be required for the formation of the parts that are to be employed in loco-motion to enable the young to go in search of food.

When the young are ready to leave the oviduct, a canal is formed for their conveyance, along which they pass; and as part of the foot is surrounded by the oviduct when the foot is protruded, that part of the oviduct will be carried beyond the external shells, so as to afford a ready escape for the young from their confinement. Before this takes place ova have again begun to form in the ovaria.

All these parts are represented in the engravings.

SECT. III.

On the Formation of Pearls.

Having in the course of the investigation of the mode of breeding of the muscle and oyster, contained in the last sections, discovered the part that forms the nucleus of a pearl, which was before unknown, and is so far important, since it becomes the source of that brilliancy from which pearls derive their beauty as well as their value:

As this nucleus in both these animals is the ovum, when by accidental circumstances it does not come in contact with the impregnating liquor, and therefore proves abortive, I have brought the consideration of pearls into this place, immediately after the account of the ova when they are impregnated.

This being a new view of the subject, it will perhaps be more clearly and satisfactorily shown, by prefacing it with the previous opinions respecting the formation of pearls, with the grounds on which they were adopted.

That pearls are formed in fresh-water muscles and oysters of different kinds has not escaped the notice of the earliest naturalists, they also had a correct idea of the mode of increase by means of annual layers of nacre; they were, however, too little acquainted with the mode of generation of the animals contained in these bivalves to form the least conjecture respecting their real source.

They observed that all shells had a power of repairing the effects of accidents, by an addition of the same materials of which they are formed; and as the nacre in the muscle and oyster gives a polish to the surfaces of the shell on which the animal occasionally moves, the projection of the pearl was believed to be the nacre deposited for the restoration of the polish to such parts of the shell as had been injured.

My friend Mr. Hatchett, at my desire many years ago, made a chemical analysis of the pearl, which has a place in the Philosophical Transactions, showing it to be composed of layers of very fine membrane, on which are deposited layers of nacre.

To prove that pearls were extraneous spherical bodies, introduced by accident or art through the shell, so as to come within the cavity in contact with the animal, which in its own defence gave them a nacral covering, specimens are preserved in the Hunterian Collection. These pearls, as they are called, were formed by drilling holes through the shells of the water-muscle while alive, and introducing in some instances shot, in others glass-beads, which the animal covered over, but the other side had no nacral covering.

The Chinese, who are the most ingenious in the arts of deceit, have brought this mode of imitating pearls to a great degree of perfection. It is done in the following manner: — They take the substance of the clamp-shell, turn it in a lathe into hemispheres of different sizes, and introduce them through the shell of the oyster with the convex surface towards the animal, the prominent part is consequently

covered with nacre, and annually receives an increase. By introducing hemispheres instead of spheres, they avoid irregularities on the opposite surface. In this manner half pearls are made, since they cannot make whole ones; and when these are set to represent pearls, they will pass off undiscovered by an unexperienced eye, but not by those who understand pearls, being deficient in lustre.

The trustees of the British Museum, at the request of Sir H. Davy and Mr. Children, have furnished me with some specimens of these artificial pearls that are exceedingly well manufactured: they are represented in the engravings.

The following remarks on the structure of pearls, and on the Chinese mode of producing them of a large size and regular form, by John Edward Gray, M.G.S., published in the Annals of Philosophy, confirm what I have stated: they are dated December 10. 1824: —

"Pearls are merely the internal pearly coat of the shell, which has assumed, from some extraneous cause, a spherical form; they are, like the shell, composed of concentric coats formed of perpendicular fibres; consequently when broken they exhibit concentric rings and fibres radiating from a central nucleus, usually consisting of a grain of sand or some other body which has irritated the animal. A pearl having been once formed, the animal continues to increase its size by the addition of fresh coats, perhaps more rapidly deposited on it than on the rest of the shell, as the prominence remains a source of irritation.

"The pearls are usually of the colour of the part of the shell to which they are attached. I have observed them

white, rose-coloured, purple *, and black, and they are said to be sometimes of a green colour; they have also been found of two colours, that is, white with a dark nucleus, which is occasioned by their being first formed on the dark margin of the shell before it is covered with the white and pearly coat of the disk, which, when it becomes extended over them and the margin, gives them that appearance.

"Pearls vary greatly in their transparency. The pink are the most transparent, and in this particular they agree with the internal coat of the shell from which they are formed, for these pearls are only formed on the pinnæ, which internally are pink and semi-transparent, and the black and purple specimens are generally more or less opaque.

"Their lustre, which is derived from the reflection of the light from their peculiar surface produced by the curious disposition of their fibres, and from their semi-transparency and form, greatly depends on the uniformity of their texture and colour of the concentric coats of which they are formed. That their lustre does depend on their radiating fibres may be distinctly proved by the inequality of the lustre of the 'Columbian pearls' which are filed out of the thick part near the hinge of the pearl oyster,

"* I can with certainty inform the anonymous author in the Edinburgh Philosophical Journal, No. xxi. p. 44., who observes, that 'in the British Museum there *is* or *was* a famous pink pearl,' that there not only now *is* one, but three of these pearls, as he might have convinced himself, for they have been exposed to the public now for these last three or four years to my own knowledge."

Avicula Margaritifera *, so that they are formed like that shell of transverse laminæ, and they consequently exhibit a plate of lustre on one side which is usually flat, and are surrounded by brilliant concentric zones, which show the places of the other plates, instead of the even beautiful soft lustre of the true pearls.

" Some time ago, in examining the shells in the British Museum, I observed a specimen of *Barbala plicata* †, with several very fine regular shaped semi-orbicular pearls of most beautiful water, and on turning to their superb collection of pearls, I found several fragments of the same shell with similar pearls, and on the attentive examination of one of them, which was cracked across, I observed it to be formed of a thick coat consisting of several concentric plates formed over a piece of mother-of-pearl roughly filed into a plano-convex form, like the top of a mother-of-pearl button. On examining the other pearls they all appeared to be formed on the same plan. In one or two places where the pearl had been destroyed or cut out, there was left in the inside of the shell a circular cavity with a flat base, about the depth, or rather less, than the thickness

" * I have placed this shell with the *Aviculæ*, as, when young, it has the teeth of that genus; and I have seen an old specimen which would scarcely agree with Lamark's ' *Cardo edentulus*.' "

" † This shell was described and figured by Dr. Leach in his Zoological Miscellany, under the name of *Dipsas plicatus*, but Dipsas has been used as a genus of Annulosa. I have, therefore, adopted Mr. Humphrey's name; Dr. Leach had changed it to *Appius plicatus*. — It may be the *Mytilus plicatus* of Solander's MSS. confounded by Dillwyn with the *Mytilus dubius* of Gmelin, but the pearls are certainly not ' furnished with stalks,' as they are described in the Portland Catalogue, p. 59. to be in that shell."

of the coat that covered the pearls, which distinctly proves that these pieces of mother-of-pearl must have been introduced when the shells were younger and thinner; and the only manner that they could have been placed in this part of the shell must be by the introduction of them between the leaf of the mantle and the internal coat of the shell; for they could not have been put in through a hole in the shell, as there was not the slightest appearance of any injury near the situation of the pearls on the outer coat.

"Since these observations I have tried the experiment of introducing some similar pieces of mother-of-pearl (which may now be truly so called) into the shell of the *Anodonta Cygneus* and *Unio Pictorum*, which I have again returned to their natural habitation; and I am in hopes that some persons who have more convenience, and are better situated for the purpose, will repeat these experiments, especially with the *Unio Margaritifera.* I found the introduction of the basis of the pearl attended with very little difficulty, and I should think very little absolute pain to the animal; for it is only necessary that the valves of the shell should be forced open to a moderate breadth, and so kept for a few seconds by means of a stop, and that then the basis should be introduced between the mantle and the shell, by slightly turning down the former part, and pushing the pieces to some little distance by means of a stick, when the stop may be withdrawn, and the animal will push the basis into a convenient place by means of its foot; and of the thirty or forty bases which I thus introduced, only one or two were pushed out again, and these I do not think had been introduced sufficiently far. In several which I afterwards

destroyed, I found that the bases were always placed near the posterior slope of the shell, where the pearls are situated in the *Barbala.*

"If this plan succeed, which I have scarcely any doubt it will, we shall be able to produce any quantity of as fine pearls as can be procured from abroad. My reason for believing that this manner of forcing the animals of the fresh-water bivalves to produce pearls, is the invention of the Chinese, a nation celebrated for their deceptions and trick, is, that in looking over the collection of shells of Mr. G. Humphreys, I observed that a shell of this species (the second perfect one that I have seen) was marked as having come from China.

"This plan at least is certainly much preferable to the one proposed by Linnæus, and by the above quoted anonymous author, as the pearls are all of a regular form, and that the one best suited for setting. In cutting these pearls from the shell, it is necessary that the shell should be cut through, so that the mother-of-pearl button may be kept in its place; for if the back were removed, as it would be were not the shell cut through, the basis would fall out, and then the pearl would be very brittle. The only objection that can be adduced against these pearls is, that their semi-orbicular and unequally coloured sides preclude them from being strung, or used any other way than set; but this fault will always be the case with all artificially produced pearls, as the mantle can only cover one side of them; and the only pearls that well answer the purpose of stringing are those found imbedded in the cells in the mantle of the animal."

Here ends Mr. Gray's account.

Since the above was written, my friend Mr. Children has pointed out to me a paragraph in the Encyclopædia Britannica, vol. vi. p. 477., in which it is stated, "Pearls are also produced by another artificial process. The shell is opened with great care to avoid injuring the animal, and a small portion of the *external surface of the shell is scraped off.* In its place is inserted a *sphærical* piece of mother-of-pearl, about the size of a *small grain of shot.* This serves as a nucleus, on which is deposited the pearly fluid, and in time forms pearl. Experiments of this kind have been made in Finland, and have been repeated in other countries."

Having stated all that was known upon the subject of the formation of pearls, previous to the time at which I took up this enquiry in the year 1824, I shall now explain what a pearl really is; and if in the course of my explanation, I shall prove that this, the richest jewel in a monarch's crown, which cannot be imitated by any art of man, either in the beauty of its form or the brilliancy and lustre produced by a central illuminated cell, is the abortive egg of an oyster enveloped in its own nacre, of which it receives annually a layer of increase during the life of the animal, who will not be struck with wonder and astonishment!

In my investigation of the mode of breeding of the oyster and muscle, when the ova were examined in the microscope, we commonly found round hard bodies too small to be noticed by the naked eye, having exactly the appearance of seed-pearls, as they are called, in the ovarium,

or connected with the surface of the shell in contact with the membrane covering it, which led me to consider this to be the situation in which pearls are originally formed, more especially as here they not only were very small, but uniformly of the same size, and when found more and more distant from this spot they had increased in size.

In looking over some East Indian pearls, several of which had been split, I was struck with the brightness of the polish of the central cell, and immediately enquired whether that was met with in all pearls, but was told it was not. This assertion I could not credit; and when the proofs of its being wanting were laid before me, I saw at once that the pearl had not been split in the centre. Examining the split surface of all the pearls within my reach, I found the cell universal; and when the size of the cell was compared with that of the ovum at the time it is ready to drop off from its pedicle, it was sufficiently large to enclose it: these facts led me to conclude that the ova which prove abortive do not die and drop off at the same time that those which have been impregnated pass into the oviduct, but remain in their capsulæ, which being still supplied with blood-vessels go on increasing for another year; their surface then receives a nacral covering with all the other surfaces of the shells, and they lose their attachment or become imbedded in the shell: this is in some measure proved by pearls being met with perfectly spherical, others in which the pedicles are included in the nacral coat, others again more or less buried in the nacral coat of the shell.

As pearls have their origin from so small a nucleus,

it is not surprising that there are so few of a large size, since it is probable that oysters of great size, which will depend upon their age, live in deep water beyond the reach of man.

In this age of speculation, when so many joint-stock companies have been formed upon the wildest schemes for accumulating wealth, and by their failure have ruined so many individuals, who have been led away by the visionary prospects held out to them, one has been to send out ships with diving bells, for the sole purpose of establishing a pearl fishery upon a great scale. I have taken advantage of this speculation, and have sent out an intelligent person on board one of these ships, with instructions to descend in the diving bell upon every occasion, and at the same time note the longitude and latitude of the place, and make memorandums of the substances within the reach of his sight from the bell at the bottom of the sea; and in this way, should his opportunities be sufficiently frequent, he may bring home materials to form the beginning of a sub-marine map, which has never before been attempted, and which may turn the avidity of man for the acquiring large fortunes by the most hazardous means, to the promotion of science.

I have been referred to the following letter in the Philosophical Transactions, 1st December, 1673, in which the discovery of pearls being produced from the ova of muscles is clearly promulgated: —

" Pearl shells in Norway do breed in sweet waters; their shells are like muscles, but larger; the fish is like an oyster, it produceth clusters of eggs; these when ripe are

cast out, and grow and become like those that cast them; but sometimes it happens that one or two of these eggs stick fast to the side of the matrix, and are not voided with the rest. These are fed by the oyster against her will, and they do grow according to the length of time into pearls of different bigness, and imprint a mark both on the fish and shell by the situation conform to its figure."

This account, sent to the Royal Society by Christophorus Sandius from Hamburgh, induced the secretary to enquire into the authority upon which the statement was made, and the answer is, My authority is Henricus Arnoldi, an ingenious and veracious Dane, having by his own experience seen it in Christiana in Norway.

So strange a story, at a time when comparative anatomy was so little known, was not likely to receive credit; but had Henricus Arnoldi added to his statement, that in making observations with the microscope he was led to this discovery, the followers of Leuwenhoek would have pursued the enquiry and established the discovery. When I laid before the Royal Society the discovery of the mode of formation of the pearl, and stated that the central cell, lined with nacre, was the cause of the brilliancy, the rays of light pervading the diaphanous substance of the pearl, I was very much astonished to find that the best informed of my philosophical friends denied the substance of a pearl being diaphanous; and they asserted that the splendour was the effect of light reflected from the external surface. One of them said this had been clearly proved by Dr. Brewster in a paper published some years ago in the Edinburgh Magazine, or some other periodical publication; that he

himself had considered the subject, and his observations entirely coincided with those of Dr. Brewster.

Upon taking a split pearl and putting a candle behind the cell, the surface of the pearl became immediately illuminated; so that the fallacy of my philosophical friend's opinion was made self-evident; and it will be seen in the engravings that the degree of transparency of a split pearl is very considerable, for by mounting one with coloured foil behind the cell, and putting a candle behind the foil, the outer convex surface became universally of a beautiful pink colour.

The error my friends fell into was taking for granted that the pearl was a solid body; and therefore considering the subject mathematically, the brilliancy must be produced by the reflection from the nacral surface; but this reasoning was entirely inapplicable when applied to a sphere that is hollow.

The oyster, from which the pearls brought to market are procured, is a different animal from the ostrea edulis, which is an article of food in this country.

I have only seen one pearl-oyster shell, brought from the Molucca islands by Lady Raffles, which is represented to show the difference of its shape from that of the common oyster.

The best description I have met with of the pearl-oyster is in Dampier's Voyage round the World, in 1685.

" At Gorgonia, twenty-five leagues from Gallo, Dampier found pearl-oysters in great plenty: they grow to loose rocks in four, five, or six fathom water, by beards or small roots like muscles; they are commonly thinner and flatter, al-

though of the same shape with the common oyster: the fish is not sweet nor wholesome, as slimy as a shell snail, tastes copperish when ate raw, and are better boiled.

"The pearls are found at the head between the meat and the shell: some have twenty or thirty seed-pearls, some one or two pretty large ones, others none; the inside of the shell is more glorious than the pearl itself: Dampier saw none in the South Seas but there."

In Dampier's Second Voyage round the World, in 1704, written by Mr. Funnel his mate, "he mentions finding in the Gulf of Nicoza, lat. ten and eleven degrees north, among a parcel of small islands, conches, clams, pearl-oysters, and another sort called the great oyster.

"The pearl-oyster, size of the common oyster, more broad and flat, hangs by a beard like muscles; the pearls are situated in the thickest part; some have five, six, or seven. The Spaniards fish for them in this gulf. The Indians go down in five to eight fathoms water, bring up from eight to twelve at a time; the men on board open them; the flesh is green; they are fat; he had eaten them boiled and stewed, and found them tolerably good.

"The great oysters are not attached; when opened one part is red as a cherry, the rest white; they are so large that one stewed is a meal for five men; the crew ate them for want of better food.

"The muscles are so large that one stewed with pepper and vinegar is enough for two men: very tolerable food."

There are many different species of pearl-oysters, and the pearls are much more brilliant that are formed in some than others; but those more fulgent are not met with of a

large size; the shells of the oyster in which they occur being of a small size, and the annual laminæ of nacre being extremely thin.

Lady Brownrigg brought home some pearls, which she saw herself brought up by the divers at Ceylon, and favoured me with some of the shells, which will be found among the plates, as a contrast with the shell from the Moluccas received from Java; and the small ones from Ceylon exceed those from Java in lustre as much as the others exceed those from Ceylon in size.

The following observations on the pearl-fishery, on the pearl-bank Arepo, off the N. W. coast of the island of Ceylon, I have been favoured with by my friend Sir Alexander Johnston, who was president of His Majesty's council in Ceylon for many years. As they are to be depended on, I have considered them deserving of being inserted in this place.

"The shape of every pearl-oyster is the same, being an irregular oval, with a segment cut off by a straight line, at the union of the two shells, the outside smooth. The pearl-oysters either grow on the sandy bottom, or on small detached pieces of rock, from which the smallest force is sufficient to separate them. They are sometimes found in ropes or cables of oysters (as they are called), of which a good diver is immediately sensible, and coils the whole into his net without breaking it. The small and middling pearls are found in the thickest part of the flesh of the oyster, near the union of the two shells, the large pearls almost loose in that part called the beard. It is owing to this situation of the large pearls that there has

been occasion to remark that those of an extraordinary size have first been brought for sale in the bazar or market. It is that the pearl-oyster being always taken gaping, the divers and crew have an opportunity of feeling with their fingers (by gagging the oyster) for the large pearls, and on finding them, of swallowing or otherwise hiding them. One hundred and fifty pearls (including seed-pearl) have been found in an oyster, and as many oysters opened without finding a pearl.

" The pearl-oyster is said to require from seven to nine years to come to maturity, but the experience of the three last years' fisheries does not corroborate this notion; nor is it reasonable to suppose, that pearls of a perfect form, of half an inch in diameter, (three of which size were found during the fishery, 1797,) could come to that maturity in nine years. It is also supposed that the oyster remains a short time only in a state of maturity: when it dies, its contents are lost in the sea. The same effect is likewise produced from their being shaken on the banks, by the operations of the divers or any other cause: this notion respecting the pearl-oyster is generally believed by the Dutch, and the natives of India conversant with the subject, and is confirmed by the increased number of empty shells found last year. If the short existence assigned to the pearl-oysters be not without foundation, the Dutch must have lost three entire generations, their last fishery having taken place in 1768, and the first of the English in 1796. The spaces of the bottom of the sea covered with and producing oysters are by no means nearer the surface

than the surrounding parts, excepting by the diminution of depth caused by the quantity of oysters. The term 'bank' means no more than the spot on which oysters grow.

"The pearl is composed of strata or scales, which are easily removed by a skilful hand, without injury to that below, which retains all its brilliancy. Pearls of a large size and perfect form, but discoloured, have been bought at a low price, but became exceedingly valuable by removing one or two of the upper coats. Jewellers divide pearls into seven sorts, bearing different names, from the most perfect to the most rugged and uneven.

"The value of the perfect pearls, such as are used in necklaces and bracelets, is hardly more than one eighth of the value of the whole.

"The mode of estimating and calculating the value of pearls is the province of the jeweller, appearing at first sight intricate, but comprehensible by a small degree of attention. The native dealers, by a process somewhat different, value pearls with as much readiness and certainty as the European jeweller, whose mode is borrowed from the Arabians, and who may have borrowed theirs from the Ceylonese, or the Ceylonese from them."

SECT. IV.

A Description of the Ovarium of the Tetrodon Mola (Lin.), *or Short Sun-Fish.*

The ovarium I am about to describe was taken out of the female sun-fish at sea, and at the time was not known whether it should be considered as a naturally formed part, or the product of disease. It was sent to Dr. Smith the superintendent of the Museum at the Cape of Good Hope, without opening into the bag which in this fish formed the ovarium, and he transmitted it to me without any examination.

In a fish of so extraordinary a form, it became a matter of curiosity to know in what manner the ovaria would be disposed of, so as to adapt them to the shape of the fish. Their position must be different from what it is in other fishes; and from the appearance of this bag, I am led to conclude that the ovarium is single, so that it may take up the least possible room in the belly of an animal, whose body is so short: it is a pity that the dimensions of the fish itself had not been stated; but nothing can exceed the beautiful arrangement of the incipient ova the bag contains, resembling exactly that of the ova of the lamprey. The ovarium in itself appears to be single; but there is a line extending throughout its whole length, distinguishing it into two lobes.

The internal structure is wholly composed of membranous processes, loose at their edge, and projecting into the cavity attached at their base by a connection to the external bag. Having many years ago received a present from Sir Joseph Banks of an engraving of this species of Tetrodon, I shall give an engraving of it here, to make the account of the ovarium more intelligible to those who have not seen any delineation of the fish.

The representation of the inside of this ovarium I believe is a new acquisition to comparative anatomy, since I have never seen exactly the same distribution of the rudiments out of which the young is to be produced, upon so large a scale.

SECT. V.

On the Organs of Generation of the Proteus.

It is a curious circumstance, that although the proteus from South Carolina should have been known to Linnæus, to whom it was sent by my friend Dr. Garden nearly a century ago, and that found in the underground lakes in Germany should have been discovered for nearly the same period, that in all that time the female in a breeding state should never have been met with; so that not only something of a mystery hung over the natural history of the different tribes of this genus of animals, but naturalists had gone so far as to deny them a place in the scale of complete animals.

In the year 1821, Mr. Rosconi published a work entitled "Amours des Salamandres Aquatiques," in which he not only boldly maintains that the proteus is a larva and not a full-grown animal, but goes out of his way to make an attack upon the late Mr. Hunter's opinion respecting this animal. He says that he obtained his information of Mr. Hunter's opinion from a friend, who had actually seen in a manuscript clandestinely shown to him by the conservator of the Hunterian Collection, that Mr. Hunter considered those protei two distinct species of animals. This manuscript was never published, and neither the conservator, Mr. Rosconi's friend, nor Mr. Rosconi himself had any

right to quote from it. The manuscript came to me as an executor of Mr. Hunter, and after having employed it for the purpose of furnishing materials for the catalogue of that invaluable collection, I destroyed it along with other papers, in compliance with a request made to me by Mr. Hunter during his life, as they were not sufficiently correct to deserve being laid before the public. Till Mr. Rosconi's work came to England, I did not know that the charge I gave to the conservator to keep these papers as private documents had ever been abused, and it was after the papers had been destroyed, that Rosconi's book came into my hands. Rosconi not only abuses Mr. Hunter for a degree of ignorance which, he says, was not to have been looked for, but pays a high compliment to La Cepede, whose sagacity had decided that all the different species of proteus are larvæ.

I never doubted from the time that Baron Cuvier published his account of the skeletons of these two species, proving them to be composed of perfectly-formed bones, that they belonged to full-grown animals; and in my account of the skeleton of the proteo saurus, published first in the Philosophical Transactions, and afterwards in a former volume of this work, I mentioned my belief in the Baron's conclusion, and in consequence of their having cupped vertebræ like fishes, I considered them as part of a distinct class of animals, deserving the name of proteus.

As these works have been many years in the hands of the public, it is possible that Mr. Rosconi, in his abuse of Mr. Hunter's ignorance, intended a portion of it for me.

My friend Dr. Leech of the British Museum put into

my hands a specimen of the Mexican proteus, in which, upon finding the vertebræ cupped, I could neither doubt of its being a full-grown animal, nor that it was a third species of proteus. As, however, the organs of generation in the state of impregnation had never been examined, when Mr. Bullock went to Mexico, I requested him to bring me specimens, if they could possibly be procured, for the purpose of settling that point.

In compliance with my request, Mr. Bullock brought me several specimens, taken in a lake three miles from the city of Mexico, the temperature of the water never below 60°, and the elevation above the sea 8000 feet. They were all caught in the month of June, at which season they are so abundant as to form the principal food of the peasantry. Mr. Bullock saw thousands in the market for sale, at the same time. This supply came from another lake called Tesenco, its elevation above the sea still higher than that near the city. He met the natives returning home from the market with sixty or seventy on a string.

Mr. Bullock could procure no information respecting them, not even the distinguishing marks between the male and female; no one could tell their food, the appearance of the ova or young. By examining their stomachs, the food was ascertained to be snails and shrimps.

As the male and female organs of generation are the only parts that have not been already laid before the public, it is of these only that I mean to give a particular account; and as the drawings are so expressive of what they are intended to show, little verbal description is required.

The external parts of these organs resemble those of the aquatic salamander in a certain degree.

In the male, at the time the testicles are developed, the protrusion of the external parts is the greatest; they are composed of numerous fine membranous plicæ, which at other times are not conspicuous. The testicles are shown in situ: they are of a more delicate texture than in the aquatic salamander; but the other viscera in the abdomen, particularly the kidneys, are nearly the same, also a large gland, which, as it does not exist in the female, is probably the vesiculæ seminales.

It is a curious circumstance, that in the coitus, which is only momentary, the orifice of the male encloses the external parts of the female, contrary to what happens in other animals.

The ova, from an examination of the construction of the oviducts, would appear to pass out singly, as in the aquatic salamander.

It is with no small satisfaction, that at the same time I am enabled to determine a point in natural history, which has agitated the minds of philosophers for more than half a century, I can repel an attack upon the sagacity of Mr. Hunter, made thirty years after his death.

SECT. VI.

On the Changes the Ovum of the Frog undergoes.

In the year 1822, I examined the progress of the formation of the chick in the egg of the pullet, illustrated by drawings from the pencil of Mr. Bauer, showing that in the ova of hot-blooded animals the first parts formed are the brain and spinal marrow. I have now brought forward a similar series on the progress of organization in the ova of cold-blooded animals, illustrated in the same manner by microscopical drawings made by the same hand.

By comparing together the first rudiments of organization in the ova of these very distinct classes of animals, I shall be able to prove that, in both, the same general principle is employed in the formation of the embryo.

This enquiry has its interest considerably increased, by the ova not being composed of similar parts.

The ova of the frog, which have been selected for this investigation, are found to have no yelk. If we examine these ova in the ovaria in which they are formed, we find them to consist of small vesicles of a dark colour; when they enter the oviducts, they enlarge in size, and acquire a gelatinous covering, which increases in quantity in their course along those tubes; but the ova can neither be said to have acquired their full size, nor to have received their proportion of jelly, till they arrive at a cavity close to the termination of each

oviduct, formed by a very considerable enlargement of those tubes, corresponding, in many respects, to the cloaca in which the pullet's egg is retained till the shell becomes hard.

These large bags, in which the oviducts of the frog terminate, when distended with ova, put on an appearance so like the enlarged horns of the uterus of the quadruped when they are filled with young ones, that they have by some anatomists been called a double uterus. This, however, is an improper appellation.

When the ova are deposited in these reservoirs, they become completely formed, and in a state to be impregnated by the male influence, which is applied to them in the act of their expulsion. As they are pressed upon each other, by being confined in a small space, the gelatinous covering takes on an hexagonal figure, in the centre of which is the ovum.

The ova, when examined by a magnifying glass in a strong light, exhibit an appearance so similar to the molecule in the pullet's egg, as to be readily mistaken for it; but a more attentive inspection shows, that it is only a white portion in the ovum seen through the covering of the vesicle. When the vesicle is punctured by the point of a needle, the contents are so fluid as readily to run out, leaving the strong transparent membranous bag lined with a fluid nigrum pigmentum, empty.

Immediately after impregnation there is no change in the appearance of the jelly, nor of the vesicle contained in it; in this respect corresponding exactly with what happens to the pullet's egg. The first change that is produced towards the

formation of an embryo is, the contents of the vesicle expand, its form changes from that of a sphere to an oval, and when cut through its contents are no longer fluid. In the act of coagulation, the central portion becomes of a lighter colour than that which surrounds it, swells out in the middle, and there is a distinct line by which the two portions are separated from one another: the central part, in its future changes, is converted into brain and spinal marrow, and after these organs have acquired a defined outline, the heart and other viscera are seen forming in the darker substance.

This does not exactly correspond with what takes place in the pullet's egg, that of the frog having no yelk. In the pullet's egg, the part within the inner circle of the molecule, when impregnated by the male, undergoes the necessary changes to form the brain and spinal marrow; the part within the outer circle forms the blood and its vessels; the supplies out of which the other organs are to be produced are afterwards derived from the yelk.

The membrane that forms the vesicle which is destined to contain the embryo when it has become a tadpole, has a power of enlargement as the embryo increases in size, and then performs the office both of the shell and of the membrane that lines it in the pullet's egg, at the same time serving as a defence to protect it, and allow of the blood being aerated.

The nigrum pigmentum lining the vesicle can only answer some secondary purpose, since it is not met with in the aquatic salamander, whose mode of breeding very closely resembles that of the frog. Upon reflecting that the frog's spawn is exposed to the scorching effect of the sun, and in

places where there is no shelter, this nigrum pigmentum may be given to the eggs as a defence for the young during its growth, which cannot be required in those of the aquatic salamander, since they are separately inclosed within the twisted leaves of water plants, and screened from the full force of the sun's rays. The plant whose leaves the aquatic salamander most generally selects to lay its eggs upon is the Polygonum persicare.

SECT. VII.

On the Female Organs of Generation of the Ornithorhyncus Hystrix.

It is a mortifying as well as a discreditable circumstance for natural knowledge, that the colony of New South Wales has been forty years under the government of Great Britain, and the mode of breeding of the ornithorhynci has not been completely made out.

The organs of generation of the female of either species have never till now come under my observation in a state of preservation, so as to enable me to describe or have a correct representation made of all their parts.

I have at last, through the hands of my friend, Robert Brown, Esquire, received a perfect specimen of the female organs of the hystrix, sent by Alexander Macleay, Esquire, colonial secretary to the government. They resemble those of the paradoxus, as far as they are represented in the fourth volume in this work; but in this specimen, neither the ovaria are so far developed as to have yelk-bags formed, nor are the Fallopian tubes arrived at their full size.

The vagina resembles exactly that of the paradoxus, and receives the penis in the same manner to throw the semen into the Fallopian tubes, and to impregnate the ova when they have arrived there.

When describing these organs in the paradoxus, I was at a loss to determine where the ovum received its albumen, not believing it to be in the vagina where the urine is likewise received, the urethra from the bladder opening at the same place with the Fallopian tubes. I see now from the corrogated folds of their canal, that it must be in these tubes the albumen is supplied; and when that process is completed, the ovum passes along the vagina to the cloaca, where it acquires its outer covering.

As it is now thirty-one years since I began this investigation, in what may be considered the most extraordinary tribe of animals in the world, it is highly satisfactory to me to have procured this specimen just as I was about to close the present work, and thus be enabled to enrich it with representations of the female organs of both species, as I had before been successful in giving a correct representation of their male organs.

At the close of this account I shall give a recapitulation of the different modes of propagating the species in the class mammalia, in the marsupial animals and the ornithorhynci, none of which have been satisfactorily explained by any of my predecessors, and the whole of which forms a series in itself the most curious in nature, terminating in the mode of generation in the bird, which has long been well understood.

In the human species the ovaria form a corpus luteum, in which the materials are collected that are to produce the ovum. This is impregnated by the semen of the male, just before it breaks from the ovarium; it then passes along the Fallopian tube into the uterus, where it becomes attached by

its membrane to the uterus, having a placenta formed for that purpose; it there becomes a full-formed child, and is then expelled from the womb, after which it is supported by the secretion from the breast of the mother in the form of milk.

In the quadrupeds that form the class mammalia, the same process takes place. In the kanguroo, the corpus luteum forms the ovum; it passes along the Fallopian tube, drops into the uterus, is there impregnated, the semen from a bifid pointed penis reaching the uterus by means of the lateral tubes; there the embryo is supplied with albumen; and when completely formed, the uterus opens at the os tincæ, and the fœtus is tilted into the marsupium, and adheres by the mouth to one of the nipples. In the koala and wombat there is no corpus luteum, but yelk-bags are formed in the ovaria. There are two uteri, each having one Fallopian tube and one ovarium. There are two lateral canals, one belonging to each uterus, which opens into the neck of the uterus instead of the fundus. In some instances, both uteri have been found impregnated, but the ova were of different sizes; the fœtuses pass into the marsupium, as in the kanguroo. The penis is bifid, and has a slit between the two ends of the glans, with a groove leading to each of them; there are hook-like processes on the surface of each glans to retain them in the vagina.

In the American opossum, yelk-bags form in the ovaria which are double, as also the Fallopian tubes, the uterus, single; the lateral canals double; the young passes from the uterus into the marsupium. The penis is bifid and

impregnates the ovum through the lateral canals which open into the uterus, as in the koala.

The ornithorhynci have yelk-bags formed in the ovaria. These pass into the Fallopian tubes, which unite, or nearly so, but open by separate orifices into the vagina, just behind the neck of the bladder where the bifid penis is introduced, and by the apertures at the glans injects the semen into the Fallopian tubes. The penis passing into the vagina or oviduct, is something corresponding to the bird; as the penis of the drake, of which I have before given a representation. The ovum passes after impregnation into the vagina, and thence into the cloaca, where it receives a shell, as in the bird; the urethra opens into the vagina. These organs in the paradoxus are the same as in the hystrix: the cloaca is the same in both species. They have no marsupial bag, and in the hystrix there are certainly no nipples; nor can I believe there are any in the paradoxus, in which the yelk-bags, and all the other parts, correspond so exactly with those of the hystrix.

In a splendid description of the paradoxus, in folio, entitled, "Ornithorhynci Paradoxi Descriptio Anatomica, Auctore Joanne Frederico Meckelio, Lipsiæ," the professor gives the representation of a large tubular structure on each side of the belly, with a central external opening to each, which he considers to be the mammæ. To what purpose these organs can be applied, or whether they are natural parts, I am unable to decide; but their not existing in the hystrix, makes me suspend my judgment with respect to their use.

SECT. VIII.

On the Placenta.

At the end of the third volume, I had gone so far in the investigation of this organ, as to ascertain that its form and structure were different in every two genera of the class mammalia in which it is met with; and I had reason to believe, that notwithstanding its form in the species of the same genus had a close resemblance, there were certain points in which they differed, and it was such slight differences that accounted for the length of utero-gestation not being the same, it being one month longer in the horse than in the ass.

Since that time I discovered the nerves in the placenta, which discovery has a place in the present volume, and an explanation given of the many lights which are thereby thrown upon the process of utero-gestation.

I have now, just as this volume was in the press, received the placenta of the hyrax capensis from my friend Dr. Smith, who had the superintendence of the museum, established for some years under the protection of Government at the Cape of Good Hope, but which I am sorry to say is now given up from too narrow notions of economy, just at the time it was beginning, under the exertions of Dr. Smith, to show the advantage of such an establishment to the promotion of science.

This animal, the hyrax capensis, is a new genus, allied in some of its habits to the cavii. It burrows in the ground, and is very common in the caverns that are numerous in the Table Mountain. It lives on vegetables, and is easily tamed, as I am informed from one who kept them in that state while he resided at the Cape.

The placenta is annular, like that in the cat; and yet it appears to be made up of five separate lobes, each of which sends off separate arterial and venal branches, by the junction of which the navel-string is formed. A more particular description will be found in the explanation of the plates.

This led me to consider the placenta generally, and at the same time to take a view of the habits of the mother in whose uterus it is met with, and see how far the one was adapted to the other in having a relation to the security of the placenta, while the mother is engaged in such actions as are required for procuring food, concealing herself from her enemies, or in self-defence when attacked.

The first observation that occurred was, that the situation of their nerves is in all cases to be explained as intended for their security from accidental pressure, they being so completely embedded in the surrounding parts; and this is the reason that they were not sooner discovered, no such nicety being required either with respect to the arteries or veins. This I shall illustrate in the human navel-string, in which the arterial and venal trunks are so prominent as to be unusually exposed, but the nerves are in the angle between them so as to be secure from the possibility of being pressed upon.

If we carry this enquiry into the difference met with in

the form of the human placenta and that of the monkey, the next genus in the order of nature, we find the first is made up of one mass, and the embryo is appended from it, the habits of the mother during pregnancy being those of remaining quiet, or at least making no exertions of the body beyond its being gently exercised. In the monkey, the placenta is spread over double the quantity of surface, is thinner, and the navel-string is longer; its substance is completely divided into two lobes, so that it may be said to be a double placenta, with the blood-vessels of the one anastomosing with those of the other. The mother is from this mechanism of the placenta enabled to procure food by climbing up trees, and bounding from one tree to another, an exertion which no woman with child could perform without hazarding the life of her offspring.

The mare, which is perhaps fleeter in the race than any other animal, has, to enable it to continue its rapid progressive motion, a structure of the connecting medium between the mother and offspring, of that description which will be less liable to be hurt by accidental violence in the exertions of the mother. The placentas that have come under my observation since the publication of the third and fourth volumes, are those of the tapir, seal, and hyrax capensis. These are all very different, and, as far as we are acquainted with the habits of life of these different animals, seem to have been so formed and so placed as to interfere in the least possible degree with the convenience of the mother in the common actions of life.

In the seal, whose life is spent in the water, or when out

of it basking in the sun upon rocks at a distance from the shore, the placenta is made up of five lobules on the fœtal surface, but the fissures do not penetrate to the maternal surface.

The tapir, like the horse, has only a chorion, from which we are led to believe it is occasionally hunted, and requires all the velocity to its progressive motion which the animal can give to save it from its enemies.

The hyrax capensis, we know, is a burrowing animal. Its placenta is in the form of a zone, and encircles that part of the animal's body which is the smallest and capable of yielding readily to pressure without being injured. It is applied closely to the body, so as to make the bulk of the embryo at that part less than any other, the water in the amnion making the bag swell out at the two ends. The placenta has also its edges defended, while the animal is passing through small holes, from pressure, by the bones of the shoulders and the bones of the pelvis. The lion and cat kind have all placentas in the form of a zone, and, when particularly examined, I have no doubt will be found to possess peculiarities by which they may be distinguished from one another, adapting them to the mode of life of the females of the different species to which they belong.

CHAPTER IX.

On the Male Organs of Animals.

Having investigated the mode of developement of the embryo after the ovum has been impregnated in every class of animals from the human species to the oyster, I shall now bring forward such facts as I have acquired respecting the male organs that were not before known, and which throw considerable light upon the process of propagating the species.

That the organs of the male are variously formed, adapting them to the parts in the female which form the passage to the ova that are to be impregnated, has been sufficiently shown in the preceding volumes, more particularly in the kangaroo, that has a bifid glans penis: although superficially examined the point appears single; and it was only by a more minute inspection I found it otherwise.

SECT. I.

On the Semen Masculinum.

Before I engaged in researches into the rudiments of which an animal is composed, I was not aware that we had so few facts upon record that can be depended upon, respecting the process of generation, or such wild theories concerning the male semen were accredited even at the present day, as that it contains living animalculæ.

Leuwenhoek, it is true, published an account of having observed in his microscope such animalculæ in the semen, and gave representations of their appearance and their motions which were very lively; he also concludes that the ova of the female are merely the nidus in which one of these animalculæ are deposited, and remaining there forms the embryo.

This opinion of animalculæ has never been entirely exploded, since in the microscope there is such appearance; and no observer has hitherto considered the subject whose anatomical knowledge could enable him to explain the fallacy by which Leuwenhoek had been misled.

Mr. Maupertuis in a dissertation on generation, published in 1752, says, that he has never seen such animalculæ, but cannot deny their existence; he rather prefers this theory to that of Hervey, only differs in opinion respecting the use to which they are applied, and asks whether it may not be

similar to that of the flies collected in the Levant for the purpose of conveying the farina or pollen of the male flower of the fig-tree to the female flower.

In 1825 and the following year, the French physiologists took up the consideration of this enquiry, and a great improvement having been made in the construction of the microscope by an ingenious artist in Paris, Mr. Chevalier, by which objects can be magnified 400 or even 600 times, giving a distinct image, they applied the use of this new instrument to the examination of the semen in man and a great variety of animals. As soon as the representations of what they had seen were brought to this country, I requested Mr. Bauer to take up the subject.

In examining the representations given in this new microscope, the size of the supposed animalculæ was enormous compared to those of Leuwenhoek, having the form of tadpoles so far as being globular at one end and terminating in a point at the other: they all had transparent spherical spots in the head, which were supposed to be the eyes; it was, however, remarkable, that in these figures no two tadpoles were exactly alike, some long, others short, some with one of these eyes, some with two, others with four: the heads with one eye were spherical, those with four had the shape of a mulberry: they were said, when first exposed on the field, to have moved very briskly, and to have continued to do so for hours, and in some instances for days; but the motion as well as the appearance itself required the secretion to be diluted with warm water.

Although Mr. Bauer's microscope fell so very short in its magnifying power to that which had been used in Paris,

he could not believe that an instrument, in which the globules of the blood, the pedicles of the red snow, and the vibriones tritici had been so distinctly seen and examined, was unable to detect appearances in the semen, which when magnified 500 diameters were two or three inches long.

He first examined the human semen masculinum, and the following is the result of his observations.

Immediately after the emission it is no sooner exposed to the air than it forms a jelly, in which nothing can be distinguished, but this dissolves spontaneously, forming a pellucid liquor which does not again coagulate. When the secretion is taken from the epididymus of the quadruped immediately after death the same changes occur, as also in the testes of the bird.

This solution in the field of the microscope put on the same appearance, when taken from the rabbit and the goose, as the human semen masculinum: it was made up of globules of the same size as those met with in the human cerebrum.

This examination was repeated, varying the circumstances in different ways, but the result was the same, whether the secretion was diluted with saliva or distilled water; whether exposed to a bright sun or when the sun was obscured with white clouds, or the object illuminated by an Argand's lamp, the globules underwent no change in their form; no appearance resembling animalculæ, although most attentively looking for it, could be perceived by Mr. Bauer, Mr. Andrews, and myself, who were present on the occasion. In watching the change that takes place when the semen

becomes liquid, small globules of air were let loose, which became more numerous when saliva was added to it.

The failure in Mr. Bauer's experiments in bringing to our view the appearance of animalculæ in the semen, however inferior the magnifying power of his microscope to that of the French, led me to the belief that no such exist, and the other observers less expert in the use of the instrument had been misled by some errors arising out of the great magnifying power of the glasses they employed.

Finding that two of my philosophical friends, in the face of the evidence that has been adduced, are satisfied from their own observation that the appearance of tadpole animalculæ in a state of animation is met with in the semen masculinum, it occurred to me that in no animal could this subject be better investigated than in the fallow deer, and as they are in great numbers in Richmond Park, which is so near Mr. Bauer's residence at Kew as to admit of the secretion of the testicle being examined in half an hour after the animal is killed, I made an arrangement with the park-keeper to send to Mr. Bauer weekly, from the 3d September, the testicles of a buck, the vasa deferentia of which were secured by ligatures before they were removed from the body, and being carried by a man on horseback in his bosom, wrapped up in warm flannel, the contents were examined while the parts continued warm. On the tenth as well as on the third, the appearance was that of a granulated fluid, made up of globules smaller than those of the blood floating in a transparent mucus. On the seventeenth the proportion of mucus appeared increased, on the twenty-fourth still more so; on the 1st October the

liquid was in such quantity as to run out when the ligature was removed or a puncture made into the epididymus, but the globules had undergone no change, the mucus had become more viscid; but while the semen was in a pure state no appearance of tadpoles at rest or in motion could be observed. As the rut was now coming on no more bucks were killed for the table; but the park-keeper had directions to geld one which was to be fattened as a heaver for the table: this was done on the seventeenth, and the appearance of the semen exactly the same as in the others, only the liquid was more viscid and more abundant, but there was no appearance of animalculæ.

That the testicles when immersed in water emit a quantity of gas was frequently observed in these experiments, and I made several ineffectual attempts to collect some of it, the elasticity forcing the stopples out of the phials; but on the 13th October, 1825, I procured a sufficient quantity to enable Professor Brande to examine its nature. He informed me that this gas did not enter into the composition of the secretion; but when the semen comes in contact with the atmospheric air, the oxygen absorbed produces carbonic acid gas; and it is probably the globules of this gas that are met with entangled in the gelatinous mucus of the semen, that puts on, when magnified in a higher degree than Mr. Bauer's microscope could produce, an appearance of tadpoles, the mucus surrounding the air globules forming tails, but without any internal motion.

Having pursued this investigation thus far, and having no means of carrying it further, I felt satisfied that if Mr. Bauer could see no tadpoles, as no observer was more

competent to detect them, this appearance must be a microscopical deception: although this was my own opinion, I was still open to conviction, still ready to examine fresh evidence whenever the opportunity might offer.

I went so far as to request Mr. Bauer to accompany me to Paris for this purpose, to which he readily assented. While there, both Mr. Chevalier the instrument maker, and Mr. Turpin the draftsman, who made the drawings from which the engravings are copied into the French publication on seminal animalculæ, showed us every civility, and allowed us to inspect the drawings, as well as specimens of animalculæ dried on glass. Some animals that are generated in paste made with flour and water were exhibited in high vivacity, moving with great rapidity, leaving no doubt respecting their life; but although an appointment was made for the purpose of seeing the animalculæ seminis alive, to our great disappointment nothing was put before us but the dried specimens on glass. Chevalier had a microscope just completed in his best manner, which I purchased and brought to England. I am indebted to Baron Humboldt for many kind attentions, but for none more than his explaining to Mr. Bauer the use of the different parts of the instrument from one in his own possession.

Upon my return to London, I got Mr. Bauer to pursue the observations on the semen of the deer in Chevalier's microscope, in the following year after the rut had begun, consequently when in the most perfect state, and shall detail his observations on it.

On the 10th October, 1826, the following experiment was

made upon the testicles of a deer in Richmond Park: the rut had commenced, and the park-keeper had left off supplying venison. Immediately after the buck was shot, the vasa deferentia were both tied, and the testicles removed. While warm from the animal a section was made upon the spot from the epididymus, and some of the secretion smeared upon glass: it was placed by Mr. Bauer in the field of Chevalier's improved microscope, and examined by him, Dr. Rodolfe Vivenot, a young physician from Vienna, well accustomed to microscopical observations, and myself; nothing was seen but an infinity of globules. A little warm water being added, there was a new appearance; the globules which were before uniformly transparent were now opaque in the circumference and transparent in the centre: they had more than doubled their size; they resembled exactly the head part of the supposed tadpoles described and delineated in the French publications on this subject; part only of the original globules had undergone this change.

Some of the secretion taken from the tubuli testis exhibited not only small globules, but also a transparent mucus spread in different shapes upon the glass, confirming all the former experiments and observations.

As Dr. Rodolfe Vivenot was going to Paris in October, and had seen all that we had met with, I gave him an introduction to Mr. Chevalier, the inventor of the microscope, requesting he would show him the animalculæ in motion. The Doctor, in his answer, says he was shown the mites in cheese, and other animalculæ, but no animalculæ seminis.

When the animalculæ infusatoriæ were examined in this microscope, their movements were perfectly distinct, and

extremely rapid, although their size was much smaller then the globules and mucus of the secretion then under consideration.

We were led, from this most favourable opportunity of making our observations, the animal being excited by nature for employing these organs to the propagation of the species, and therefore in the state to secrete a fluid fitted for that purpose; we were led, I say, to conclude, that the appearance of living animalculæ in the semen is not a real one, but is the effect of a microscopical deception. Without an opportunity of employing the same instrument in which animalculæ were said to have been seen by the French physiologists, both Mr. Bauer and myself hesitated at coming to a decision; and although I had collected evidence against their existence, when I was told by members of the council of the Royal Society that they had seen them, I withdrew my communication after it had been read at the meeting of the Society; but now, after having gone through this investigation for two seasons in the deer, I must return to my former conclusion.

SECT. II.

On the Structure of the Penis.

Nothing can show more strongly the importance of the use of the microscope, in making researches into the more minute parts of anatomy, than the advantages that have been derived from it in the present enquiry.

When I entered upon it, unless I had been assisted by Mr. Bauer, although supplied with preparations of the parts that were to be examined, made with great art, and exposing the different structures in the most satisfactory manner, I never should have succeeded in describing them, and without his pencil I could not have laid such appearances as had been seen before the public.

The structure of the human urethra, a part of so much importance, from the miseries entailed upon mankind by the diseases to which it is liable when they are not properly treated, was, literally speaking, quite unknown; it is now found to be made up of a thin membranous lining, devoid of the power of contraction, over which is a muscular coat. When a transverse section is made of the urethra in its contracted state, the internal membrane is thrown into folds or longitudinal plicæ, pressed together by the surrounding parts.

On the inner surface of this membrane there are numberless small papillæ; it is exceedingly vascular, and has a

number of orifices leading to deep-seated glands. Those appearances are accurately shown in the annexed drawings.

The muscular covering by which this membranous tube is surrounded, is composed of longitudinal fasciculi, of short muscular fibres connected together, so as to form one muscular mass or tube by means of an elastic substance, of the consistence of mucus.

This mechanism is very different from what it was contemplated to be. Till now it was believed, that either the lining of the urethra was composed of circular fibres possessed of a power of contraction, or was immediately surrounded by such fibres, and their contraction was the means by which the canal is closed.

The corpus spongiosum, and the corpora cavernosa, when in a state of collapse, which is their usual state in the dead body, do not admit of being expanded, unless by means of injection, so very elastic are the materials of which they are composed. The injections employed that will pass from the arteries into the cells of these bodies, were proof spirit and quicksilver, and when by this means distended and hardened in strong spirit, they admitted of a more accurate examination. There was also an opportunity occurred of examining these parts distended by their own blood, the patient having died while the penis was in the state of priapism.

From an examination of these different preparations, it is found that the corpora cavernosa are made up of an infinite number of membranous plates, which are exceedingly elastic, and connected together so as to form a trellis, attached firmly to a strong elastic ligament by which these bodies are surrounded, and also to the septum which lies between

them. This elastic substance has an intermixture of muscular fibres. The septum is thinner in some individuals than others, and in one specimen, at the anterior part, was almost entirely wanting.

In the central line of each corpus cavernosum, there is an open space of different extent in different specimens. It has no regular boundary, only the elastic plates or trellis-work is wanting there.

As the structure of these elastic plates is a subject on which physiologists are not agreed, Mr. Bauer took infinite pains in his examination, and in his microscopical observations upon it. He has satisfied himself that they are single, vary in size, and the interstices between them irregular, as represented in the drawings. Their surface has innumerable small arteries branching upon it, which pour their contents into the interstices when the penis is to take on a state of erection. The difference of the circulation in the penis when it is erect, and when it is not, is one of the most beautiful illustrations of the influence of the nerves upon the action of the arteries.

The corpus spongiosum urethræ differs in its structure in nothing from the corpora cavernosa, but the parts being upon a smaller scale, there being no central cavity, and no admixture of muscular fibres in the ligamentous covering by which it is surrounded. When a transverse section is made of the corpus spongiosum, and examined in the microscope, the orifices of six or seven arteries that had been divided, are distinctly seen in different parts of the section.

The corpus spongiosum is continued into the glans penis, whose structure is nearly of the same kind.

Upon the surface of the glans penis, there are many little circumscribed dots, which are for the purpose of giving the peculiar pleasurable sensation produced by gentle friction upon its surface. They are distinctly shown in the drawing in a magnified state.

The penis, in many quadrupeds, differs from the human, in having a bone in the part occupied by the corpora cavernosa. This I consider to answer the purpose of keeping up a continuance of the erection, for the time which is required to inject the semen. This is met with in most animals that copulate in water, as the sea-otter, manates, dugong, hippopotamus and walrus, but many land animals have the same provision, as all the dog kind. All such animals, I have no doubt, are unprovided with the cavity I have described in the corpora cavernosa of the human penis, such a large reservoir for the blood answering the same purpose as the bone, giving strength and permanency to the erection. This reservoir is very large in the penis of the land tortoise, and in that animal I first observed it.

SECT. III.

On the Structure of the Prostate Gland.

It was in the year 1806, that I laid before the Royal Society the discovery of a third lobe of the prostate gland, and as it did not, in any way, belong to the particular subjects on which I gave lectures in comparative anatomy, although in itself probably of more importance to the healing art than the result of any of my other investigations connected with anatomy, I have passed it over in the preceding volumes; but as many of the investigations, in this supplementary part, are on the structure of the organs of generation, I shall give it a place in this volume, although in another work connected with the treatment of diseases of those organs, I have considered the subject more at large.

The more minute parts of many organs of the body have been very lightly considered, by those who were very deficient in their knowledge of anatomy; and it will appear, that till this discovery was made, the source of a most important disease had been overlooked, and consequently the nature of some of the symptoms entirely misunderstood.

The form of this gland has not been unaptly compared to the figure of the ace of hearts, as it is represented on cards, and when one side of it only is exposed, I know of no better resemblance.

Mr. Hunter came very near the discovery which I am about to claim, and had he pursued the investigation, would certainly have arrived at it, as it was only by doing so that the discovery was made. As I have no wish to assume to myself more than belongs to me, I shall now mention what Mr. Hunter published on the subject. In his work on the venereal disease, he says, page 169, " From the situation of the gland, which is principally on the two sides of the canal, and but little if at all on the fore part, as also very little on the posterior side, when it swells it can only be laterally, whereby it presses the two sides of the canal together, and at the same time stretches it from the anterior edge or side to the posterior, so that the canal, instead of being round, is flattened into a narrow groove. Sometimes the gland swells more on one side than the other, which makes an obliquity in the canal passing through it.

" Besides this effect of the lateral parts swelling, a small portion of it, which is behind the very beginning of the urethra, swells forward like a point into the bladder, acting like a valve to the mouth of the urethra, which can be seen even when the swelling is not considerable, by looking on the mouth of the urethra from the cavity of the bladder in the dead body. It sometimes increases so much as to form a tumour projecting into the cavity of the bladder some inches."

When Mr. Hunter asserts that when the prostate gland swells, it can only be laterally, it is clear that he had no knowledge of a regularly-formed lobe lying between the two lateral lobes, uniting them together, capable of en-

larging independently of the lateral lobes, and also of subsiding after it had so enlarged.

Although for many years a considerable portion of my time had been employed in relieving patients, labouring under complaints connected with enlargement of this gland, and I had ample opportunities of examining the state of the parts in the dead body, my attention had been so much employed in endeavouring to arrive at perfection in the operation of drawing off the water, that it did not occur to me to institute an enquiry into the origin of the disease, from whence the difficulty of making water had been produced, till the month of December, 1805.

At that time my attention was drawn to this subject by the following circumstances. In the examination of the gland, after death, of an elderly person who had died in consequence of being unable to pass his water, I found a projecting very regularly formed nipple, directly behind the inner membrane of the bladder, from which a bridle or fold of a membrane, nearly a quarter of an inch broad, extended to the bulb of the urethra, forming a projecting ridge which there terminated, and in this fold was concealed the usually rounded projection of the caput gallinaginis, or rather the remains of it, for it had wasted away; the length of this bridle was inconsiderable, although the distance commonly met with between the base of the projection into the bladder, and the verumontanum was extended to twice its natural length, so that in this instance the bridle seemed to have drawn the bulb of the urethra towards the projection in the bladder, and considerably to have shortened the membranous portion of the urethra.

This unusual alteration of structure, forming the bridle, I found had not been observed by other surgeons, and appeared to deserve particular attention. The bridle was evidently formed by the inner membrane of the bladder adhering firmly to that part of the prostate gland projecting into the bladder, which it consequently followed in the future increase, and drew after it the membrane of the urethra, and in this way the bridle had acquired its breadth, and continued itself to the bulb, where the lining of the urethra, adhering more firmly to the parts behind, could not be raised. I have since found that the appearance of a bridle is a common one in such cases, but rarely so conspicuous.

To satisfy myself how this tumid part was formed, it became necessary to examine the prostate gland accurately in its healthy state, and ascertain whether it was a natural appearance, or wholly the consequence of changes produced by disease.

My professional avocations not affording time to repeat the dissections necessary in this investigation, Mr. Brodie, who at that time was demonstrator to the School of Anatomy in Windmill Street, took the whole of that labour upon himself.

Having moderately distended the urinary bladder with water, and exposed the vesiculæ seminales and vasa deferentia, these vesiculæ were carefully separated by the knife, from their connection with the posterior coats of the bladder, and turned down upon the prostate gland where it embraces the canal of the urethra in its passage from the bladder; the posterior portions of the two lateral lobes were care-

fully exposed, and they were found to be united together by a rounded body, so like in its size and appearance to Cooper's glands in the same subject, that I was induced to believe it to be a distinct body, but on a closer inspection no excretory duct could be discovered, nor could it be wholly detached from the lateral lobes of the prostate.

These parts were carefully examined in five different bodies; but similar to what happens in respect to Cooper's glands, in no two subjects was the same appearance met with: in some it was large, round, and prominent, as in the annexed plate, in some small, and in some almost entirely wasted away. In the body of a person twenty-five years of age, it was seen best, and I shall from that subject take my description. On turning back the vasa deferentia, and vesiculæ seminales, we came down to a rounded prominent body exactly in the sulcus: between the lateral lobes of the prostate, it was imbedded under their rounded termination, and adhered to the coats of the bladder: its circumference could not be distinctly seen till the ends of the lateral lobes were detached, and when this was done the continuity of the glandular substance could be readily shown on the side next the bladder; the ducts passed directly through the membrane of the bladder. This lobe completes the circular ring through which the tube of the urethra passes.

Previous to this examination, it was not known to me or the other teachers of anatomy in London that such a lobe existed, or that any part of the prostate was situated between the bladder and the vasa deferentia. As this discovery has now been promulgated for so many years

among medical men, and no one has laid claim to the discovery, I need not dwell at present upon the different modes in which the form and structure of this gland has been described by former authors.

It is a discovery of so much importance, when connected with the symptoms attendant upon the enlargement of the prostate gland, that I considered it deserving of being registered in this work, the only one upon Human or Comparative Anatomy which I have published, and which contains all the other investigations I have made upon the structure of the human body and that of animals.

CHAPTER X.

On Diet; or, Hints on the best Mode of repairing the Waste that takes place in the Human Body.

There is no subject of so much importance to the health of the body and the vigour of the mind, as the consideration of the supplies by which the waste of the body is repaired. These consist of animal and vegetable substances in different states of preparation.

Till I was acquainted with the different materials of which the various organs of animals are composed, and the mode in which they are distributed, I was unable to give my mind to the consideration of diet. While the blood was considered to be a fluid in which none of the integral parts of the brain and muscles existed, but out of which they were generated, there were no data to reason from respecting what could be the best substances for the formation of such a fluid: we had no rule to go by except making experiments and drawing conclusions from the results; but if the investigations, which have been brought forward in these volumes, are to be depended on, and the blood is really made up of so many different parts, each of them appropriated for the repairs of the waste that takes

place in particular organs, and that the nerves are the agents that adapt the quantities of the supplies to the waste produced, then we can understand that one kind of diet may strengthen the powers of some organs, and another kind those of others.

We have seen, in the examination of the structure of the stomachs of different tribes of animals, that the organization, by which the process of digestion, or that which converts the dead animal and vegetable matter into chyle, is very simple, and that the various wise contrivances there met with are only for the preparation of particular species of food, to make them more readily go through the ultimate change which is to be produced by the process of digestion.

By means of these additions, the stomach is found not to be limited in its powers to the digestion of any one species of food, but is capable of extracting or producing chyle from the whole range of the animal and vegetable kingdoms, when deprived of its more appropriate food; such are the provisions of nature for the preservation of animal life, and to prevent the different species of animals from becoming extinct.

The present consideration is not on what species of food man and animals can subsist, but on those species which are best adapted to give them powers to fulfil, in the best possible manner, the functions for which they are intended, and as applied to man, to improve, in the greatest degree, his bodily strength and mental exertion.

We know that an eagle, whose natural food is the muscular flesh of other birds or fishes, can be gradually brought

to live on farinaceous matter, since the experiment was made by Mr. Hunter; but the eagle was so dissatisfied, that it broke its chain and flew away.

Black cattle, when deprived of grass, are found to subsist on fishes, but no longer than while denied the use of vegetables.

It has been generally considered that the teeth of animals point out their natural food, and that, judging in this way, man was at his original formation, from the structure of his teeth, intended to eat both animal and vegetable substances; this, however, is not borne out, since there are fruits and seeds which require all the peculiarities that belong to the set of teeth given to man to enable him to masticate vegetable food.

In the history of man in the Bible, we are told that dominion over the animal world was bestowed upon him at his creation, — but the divine permission to indulge in animal food was not given till after the flood. The observations I have to make accord strongly with this tradition; for while mankind remained in a state of innocence, there is every ground to believe, that their only food was the produce of the vegetable kingdom, which I shall endeavour to show is that which is principally employed in the repairs of the brain, the degree of strength, which is the result of living on flesh, not then being required in men living peaceably, and no one ambitious of invading his neighbour, and of excelling others in his bodily strength. All that has hitherto been written on the subject of diet must be confined to the results of different experiments made by individuals; and as the various substances by which man

is nourished are so numerous, and taken in such different proportions, it is hardly possible to come at any conclusions whatever, and most certainly none that can be depended on.

Were the constitutions of all men the same, we should be enabled to make our researches in this investigation with more satisfaction; but as longevity, bodily strength, and sound understanding, run in particular families, however differently the individuals of which it is composed have been nourished, we must take that circumstance into the account, and are left without any accurate knowledge of what diet alone is capable of producing.

It is true that experience has taught us some facts respecting unwholesome diet, and long continued privation of a sufficiency of food, where great numbers of individuals are placed in the same circumstances; as the effect, however different in degree, is more or less in all of them the same, such facts are to be depended upon equally with the result of any experiments made upon a large scale on any other subject.

One of these is, that the use of salt meat for a long continuance, without vegetable substances, is not only injurious to health and destructive to muscular strength, but even makes the union between the portions of broken bones, which had taken place many years before, give way again. The same experience has established the fact, that the use of acetic acid prevents this very serious malady from taking place; but till we had acquired a more enlarged knowledge of the animal economy, the cause of the mischief, as well as the mode in which it is remedied, could not be explained.

SECT. I.

On the Diet best fitted for giving Muscular Strength.

In those ages when men were raised by their superiority in bodily strength to be the lords of the world, and the rulers of mankind, diet was a subject of the greatest importance, and one to which the attention was called in no common degree.

Men were taught by experience that animal food was requisite for great feats of strength and bodily exertion, and this appeared to them somewhat in proportion to the quantity of flesh that was consumed. In children, marrow and fat were employed to produce strength; at that early age the stomach not taking so kindly to the use of the animal fibre. Chiron is said to have fed Achilles, in his infancy, upon the marrow of lions and wild bulls.

Andromache complains that, after Hector's death, Astyanax will no longer be fed by the guests at the feasts upon marrow.

In the Iliad of Homer, all the heroes were expert in killing the animals for the use of the table, and of selecting the prime pieces of the meat: they took care in broiling them that the juices should be retained, and the most valiant were helped to the largest shares.

The discoveries that have been made of the integral parts of muscular fibres completely confirm the opinions

formed by experience among the Grecian warriors; and we find that muscular flesh, which is composed of blood globules, is very readily converted by the process of digestion into muscles again; and muscular strength is repaired by such food more than by any other article of diet.

Till very lately, our proofs of the effects of this diet were confined to animals, and we could only illustrate it from the beasts of prey who were ordained after the fall of man to live wholly on animal food, with no other condiment than the blood being still warmed by life. That such animals possess more muscular strength than any others is a generally established fact; that their bones are of a more compact texture, to resist the powerful contraction of such muscles, is also well known: the bones of the fore-leg of the lion can strike fire with steel. In no country known to Europeans were the inhabitants believed to live upon a similar diet, so as to enable us to make a comparison with respect to the strength with which it would endow such men; we have, however, lately discovered nations among the natives of South America who have not any other food, which is also devoured under nearly the same circumstances that it is by the lion. Their drink is only water.

These nations have been made known to us by the great revolution which has set free the Southern American colonies from the dominion of Old Spain; and the English nation, from a desire to acquire the possession of the gold and silver, by which the Spaniards have been so long enriched from the numerous mines in that country, have speculated to the amount of many millions of British capital; and it is by the communication that has been

opened by these speculators and their agents in this expenditure, our knowledge has been acquired.

The Great Plain or Pampas, on the east of the Cordilleras, is about 900 miles in breadth; and the parts visited by Europeans, though under the same latitude, are divided into regions of different climates, from the difference of the productions. The first of these extends 180 miles from Buenos Ayres, covered with clover and thistles; the second extends 450 miles, which produces long grass; the third extends to the base of the Cordilleras, and is covered with trees and shrubs.

The south part of the Pampas is inhabited by the Pampa Indians: they wander with their cattle, and have no settled abode, but stay while there is herbage for them, and go from place to place. The north, and other provinces of Rio de la Plata, are inhabited by small groups, whose towns and villages are built for mining purposes, and are 700 miles apart.

The Guachos are many of them descended from the best families of Spain: they live in the huts in which they were born. The children at four years old are able to ride and bring home the cattle.

Captain Head states, that the Guacho who inhabits the Pampas lives entirely on mares' flesh and water, sleeps on the ground in the open air, even when it is covered with hoar-frost, and goes quite naked: from infancy he undergoes great fatigue on horseback, and rides without saddles. These Indians are a handsome, stout race of men, ferocious but brave: they never grow fat, are very athletic, lift weights and carry loads too heavy for the strongest Cornish

miners: they wear nothing on their heads: while galloping in the sun, the rapidity of their motion forms a current of air which prevents an attack of coup-de-soleil: as a luxury, they wash their hair with mares' blood.

The mares' flesh they eat is tough and lean, so that they only satisfy hunger; but when they accidentally get a buffalo, and indulge too much in eating fat, it makes them feverish, and takes away their appetite. By fasting a day or two they get well. They appear to be liable to no diseases. Captain Head saw only one woman out of health in all South America. They have no fixed abodes. Every body sleeps from twelve o'clock at noon till five. They have no peaceful habits; no arts but riding on horseback and throwing the spear. Their country supplies them abundantly with wild horses: they feed on the females, and tyrannise over the males. They despise social life. All other men are their sworn enemies. They make no prisoners, but delight in slaughter. They only hunt when hungry. They are fond of war, but never fight except on horseback; indeed they never leave their horses: they scorn to walk on foot.

Captain Head, after living for three months on flesh and water, constantly on horseback, became so hardy as to tire ten or twelve horses a day, and galloped 153 miles without halting; remaining on horseback fourteen hours and a half before he arrived at the end of his journey.

A French gentleman, whom I met in London, told me that he was, part of the time he resided in South America, of Captain Head's party; and he himself, though a slim man, after living some months on flesh and water, rode 100 miles a day without fatigue.

A friend of Dr. Babington's, who lived in the Pampas for some time as a missionary, assured the Doctor that he was astonished to find, that upon this simple diet he was able to ride more than 100 miles daily, without feeling any fatigue.

Captain Franklin, in his expedition to explore North America, mentions that the Indian warriors employed by the Hudson's Bay Company are fed entirely upon buffalo and musk ox,—the daily allowance five pounds per man: they drink nothing but water: they are very athletic, but never grow fat. This probably arises from the nature of the food making it pass readily out of the bowels, which will explain why beasts of prey never become fat.

The Esquimaux live on the cetaceous tribe, and fishes that contain oil, generally half putrid, in which state the natives had the greatest relish for it, and this even after having indulged for months in all the delicacies of English luxury. This I ascertained from some Esquimaux brought to England by Captain Cartwright, who were put under my care by Sir Joseph Banks. When they went back, Captain Cartwright told me, that before they had joined the tribe to which they belonged, they smelled a half putrid whale about half a mile from the road, when they immediately set off, men and women, to regale themselves.

They were short, and disposed to corpulency, which is the case with all the Esquimaux, and with all the North Americans on the coast that live upon fish, but they are not athletic.

The inhabitants of Fair Isle, and those islands close to it among the Hebrides, Mr. Bullock, who has been

there, informs me, live entirely upon fish, and they even feed the infants upon food made of fish and water: they are very short, the men not exceeding five feet and an inch in stature.

The Patagonians in the Straits of Magellan, who live almost wholly on shell-fish or sea-worms, although a large race of people, so far as respects the head and body, have limbs absolutely diminutive. Captain Dampier, in his account, mentions that, although they stand six feet one or two inches, their stirrups are too short for English sailors.

From these facts, which have so recently been brought to our knowledge, it is evident that the flesh of quadrupeds is more readily converted into muscular substance by digestion than any other species of food.

When we contrast the food of the aborigines of the countries north and south of our globe, the strength of their bodies, the ferociousness of their minds, with the habits, manners, and food of the aborigines near the equator, who live in Nigritia, the hottest part of the earth under the ecliptic; their diet corn and milk; their employment agriculture; their manners humane, the people gentle and docile, the same now as in ancient times, when Jupiter is said to have spent nine days every year amongst them, as being the only part of the human species fit for gods to live among; so at least Homer sung, and so Park, in our own day, reports them to be:

Bullock tells me that in Mexico, which is just within the tropics, lying in 19° 25′ N. latitude, and 42° 25′ from the ecliptic, the inhabitants, who live upon vegetable food and drink bulgue, a liquor fermented from the American wheat,

which produces no alcohol, only carbonic acid gas and saccharine matter, are equally mild in their habits with the negroes in Ethiopia :

When we make the same comparison with the animals that live upon flesh and those confined to vegetable food, we must consider that the savage disposition of the lion and the Pampans of South America is the result of the nature of their diet; and the mild disposition of the Ethiopians, the Mexicans, and of the elephant, is dependent on the same cause. The indocility of the rhinoceros is only to be explained from the small proportion of brain with which it is endowed, and therefore like all idiots intractable.

Having established the fact, that muscular fibre is the diet of all others the most productive of muscular strength, that of land quadrupeds the most so, the next thing is to enquire what vegetable substances are to be preferred, as being best adapted to give muscular strength.

In the digestion of animal fibre, little else is required than the gastric juice, and that quantity of liquid, of which water appears to be the best, for making the compost of the most proper consistence; but in considering this process, so far as regards vegetable substances, we must take into account the peculiarities belonging to individual stomachs, and adapt the kind of vegetable to them whatever they may be. There are many stomachs in which bread goes through the acetous fermentation, but carrots, parsnips, and potatoes have no such tendency, bread is therefore in such individuals to be disused: where there is no such idiosyncrasy to forbid it, bread made of the farinaceous matter of the best wheat appears to be the most nutritious

of all the articles of vegetable diet; and as solid substances, or those approaching to solids, are in general more readily digested than liquids, it is right to give the food principally in a solid form. Next to wheaten farina, I believe that of the potato is to be preferred, as it is the nearest approach to it.

There is an advantage in the potato, which, in some instances, will make it superior to wheaten bread; that is, when the supply of animal food is sufficient, or even too great, since the stomach requires not only the gratification of having nourishing food received into it, but also requires being distended in a moderate degree to satisfy the appetite: this bulk is better given by potatoes, which have about one third of the nutriment contained in bread. That this feel of emptiness is to be removed before hunger can be overcome, even where the animal food and bread are given in quite sufficient quantity for nourishment, I shall illustrate by example. A Highland regiment accustomed to feed on oatmeal in different forms, which contains little nutriment, came to England, and were put upon the rations given to the English soldier, which at that time, in consequence of the mutiny in the navy, much exceeded what was usual, being two pounds of bread and one of beef: they were dissatisfied, complained of not having had a bellyfull; and when money was given them by the Colonel's wife, they all ran to the baker's shop. If these men had worn tight belts round their stomachs, the pressure would have equally removed their sense of hunger. By this practice the faquirs in India contrive to fast for days, tying a board tight upon the stomach, and daily increasing the pressure.

In Scotland, where they live upon broths and oatmeal, from this food containing little nutriment, and producing heartburn, and letting loose in the stomach an inordinate quantity of fixed air, the cavity becomes so capacious, that they tell a story of one of the shearers who came to breakfast before his companions, eating the whole mess intended for six people, amounting to thirty-two English pints of porridge and milk: in this state he could not walk upright.

In London a child about two years old was left in a room by itself with a large apple pudding, the whole of which was gone when the nurse returned, and the child died next day in convulsions. Upon opening the body, I found the stomach so completely distended, as to resemble a very large apple pudding, tight as a drum.

When the stomach receives only animal food, and that not much dressed, it appears to require no condiments to assist digestion.

We have stated that the North American Indians do not even use salt with their food. The ancient warriors of Greece used wine with spices as a condiment; and something of this kind is necessary in all weak stomachs, to prevent them from being filled with air, under which circumstances digestion cannot go on.

In cold countries, and where vegetable food is much used, spirit of various kinds is the most favourite condiment.

Where the bulk of the food is vegetable, the best condiment is a small portion of animal substance highly seasoned with stimulating ingredients, as pickled pork; or a red-herring, and different kinds of spices; and this is

the common food of all the plantation negroes in the West Indies, except in the season when the sugar-cane is ripe, and in which they are allowed to indulge: it is more nutritious than many other vegetables. Rice is the vegetable substance more universally used as food than any other; macaroni, which is flour and water, is probably the next; and potatoes the third; but all these require condiments.

Feeding young people partly on animal food, will give them the strongest muscular fibre, which will be rendered weaker as the proportion is lessened. The strength of muscles is readily ascertained in man by making him bend the fore arm with considerable force, and then by grasping the belly of the biceps flexor muscle, which in that state, in a strong man, will feel like a piece of solid metal, and becomes a criterion of the state of his other muscles.

There is a disadvantage in feeding children upon broths or vegetables that are not of a nourishing nature, the quantity required to support the strength being so great as to over-distend the stomach, producing what is termed pot bellies: such diet renders the stomach liable to be filled with air when not supplied with food. It has another disadvantage; for as we have no method of ascertaining that the stomach is satisfied or hunger repressed, but from the internal feel of its distention, when it has been accustomed to be over-distended, it allows of food being received into it, till the muscular coats, not only of the stomach, but of the belly, lose their tone; under which circumstances digestion cannot go on well, the muscles of the abdomen yield to the least pressure, and the tendons being relaxed make the individual liable to ruptures: these muscles do not keep

the stomach in its place, much less give it support, so as to allow its own parietes to act upon the food, either in the cardiac or pyloric portion.

This is one of the causes of the number of pot-bellied sickly children that occur in Scotland, and even those of Scotch parents in England, where the same meagre, washy diet is adhered to, such as barley-broth and porridge.

The best food for children, in my opinion, is wheaten bread and milk, rice and milk, with a certain portion of animal substance once in the day. It may indeed be rendered still more so by the addition of butter; but as this species of food goes into a very small compass, and a greater quantity of nutritious matter may be conveyed into the stomach than is required for the child's support, it becomes a question whether in children potatoes and butter-milk may not be a more healthful diet, since the same quantity of potatoes contains so much less nutritious matter than wheaten bread, there is bulk to fill the stomach, and not too much nourishment to oppress the different functions of the animal economy.

If these observations are correct, the Scotch are, as a nation, neither so well nourished nor so strong as the Irish.

The Irish, from eating less animal food than the English, although not wanting in sufficient nutriment for growth, have not the same bottom, as it is termed, although larger in stature.

The French and Italians, who have less animal food, although perhaps more oil, and less bread, their vegetables being of a looser texture than corn, and their climate not leading to feats of hardihood, which is to be considered as

necessary for acquiring strength, have less strength, or at least can support the exertion of the strength they have for a less continuance than the Scotch.

That the French have less physical powers, and are less able to undergo continued fatigue, than the natives of the British Isles, was proved at the Battle of Waterloo, at Blenheim, and Cressy. Shakspeare had this notion, and makes King Henry the Fifth in the siege of Harfleur encourage his men,

> Once more unto the breach, dear friends . . . .
> . . . . And you, good yeomen,
> Whose limbs were made in England, show us here
> The mettle of your pasture. . . . .

The Roast Beef of Old England is a song implying it to be a most invigorating diet.

I was told by the late Sir Stamford Raffles, that the natives in every part of India in which he had been, whose food is rice with condiments, are less able to bear fatigue than Europeans. The farina of potatoes must be more easily saturated with the gastric juice, and admit of more ready digestion than rice.

These observations apply equally to animals. The elephants used in war in India are fed on bread.

The condiments, as has been observed, act on the nerves, but afford no nourishment: in this respect, the common people of England, more especially London, have an advantage in their beverage, which is both a condiment and a nourishing liquor, called porter. The malt forms the nutrimental part, the hops the condiment, and in the fermentation necessary for brewing, there is so much alcohol formed,

that if a red-hot poker is plunged into a pot of porter of the best kind, there is immediately a lambent flame between the heated poker and the liquor. Ale is more intoxicating.

A roll, steeped in porter, will enable a horse to bear the same exertion longer by hours than without such cordial. In the West Indies, the convalescent soldiers recovered faster when allowed porter, than rum and water.

When under twenty years of age, so weak was my muscular strength, that being engaged in dissecting a dead elephant at Sir Ashton Lever's Museum, I drank two quarts of porter in one forenoon, and am convinced that without such cordial I could not have sustained the fatigue I went through.

The coal-heavers employed to raise the coals from the holds of the ships, make perhaps greater exertion of their physical strength than is done in any other employment: while at work, each man commonly drinks several gallons of that liquor, not only without injury to his constitution, but his strength is invigorated without much other food.

Ale has a tendency to produce corpulency, in consequence of confining the bowels, which is not the case with porter; it also contains more alcohol.

All spirituous liquors of certain intensity interfere with the process of digestion; but taken in small quantities they will correct a morbid state of that organ, and stimulate both the nerves and muscular fibres to action; and in many states of stomach they become necessary to enable it to retain particular kinds of luscious food, to which it has not been accustomed.

When taken in large quantity, they put an entire stop

to digestion. As none of the alcohol passes off by the lymphatic vessels from the cardiac portion of the stomach, it probably paralyses the muscular fibres that separate by their contraction the cardiac from the pyloric cavity, so that their contents are mixed together, and reduced to a state unfit to undergo the process of digestion. That this process is not carried on under such circumstances, is evident, since several hours after the effects of intoxication have ceased, sickness takes place, and the contents of the stomach are brought up, by the first effort to vomit, in the same state they had been received; while in cases of sickness under ordinary circumstances, the first effort only brings up the contents of the cardiac cavity, and it is not till after repeated efforts that the contents of the pyloric cavity are discharged.

It is a curious circumstance, and by no means uncommon, that several fishes, certain species of the mollusca or sea-worms, and some fruits, as strawberries, act as poisons on particular stomachs. What this arises from it is difficult to conjecture, unless it is the essential oil acting on the nerves of the cardiac portion of the stomach.

Drunkards rarely grow fat: this, however, is probably in consequence of alcohol producing disease in the liver, and vitiating its secretions, so that the intestines are not supplied with healthy bile, without which fat cannot be formed in the lower bowels.

Before the lymphatic vessels of the cardiac portion of the stomach were discovered, rhubarb was exhibited as a medicine to prevent the formation of gall-stones; but when such calculi are analysed, they are found to have a resinous

matter for their base: this circumstance renders rhubarb, which contains resin, the most improper medicine to be employed on such occasions.

As the progeny is not a fresh creation, according to my mode of viewing the subject, but a portion of the brain of the parents evolved into an offspring, the young must, as is found to be the case, receive a certain stamp on its constitution from its parents. This fact is well established in horses, and is called blood. In race-horses it gives a value to the colt at its birth, provided the pedigree of the parents can be traced without any stain; and there is no doubt that it is, or ought to be made requisite, that the mother suckles its colt. Great light is thrown upon this subject by the discovery of the nerves of the placenta, and it convinces me that every healthy mother should give to her child the diet most congenial to it, mother's milk.

From these observations, strength of body is proved to be in part dependent upon the original stock, although it may be much improved by diet and regular exercise in good air: these in themselves will go far in rendering delicate constitutions more robust; but where the original stamina are good, the sparest diet will not prevent their complete development.

Such are the resources within the animal economy, that, when the stomach is in health, sufficient nutriment can be extracted from the sparest diet, for the preservation of health, and keeping up the bodily strength; while, on the contrary, under circumstances of disease or anxiety of mind, the most generous diet shall be unable to prevent the body from falling into decay.

Sleep is considered as being favourable to digestion; but the proportion that animals require is small when compared with what is necessary for mankind, since a state of rest will admit of the muscles of the body being recruited, and it is only the wear and tear of the organs of sense, which are seldom unoccupied while the animal is awake, that makes sleep necessary in them for keeping up the repairs of those organs. The simple contraction of muscles, when not controlled by the will, produces little waste, since in some diseases voluntary muscles act during sleep, but their action is without energy.

The diet must always be in proportion to the exertions of the muscles, when they are continued for any length of time; but a very small proportion of food is necessary to support the body when at rest. Prisoners living on bread and water, lying on the ground, and resigned to their situation, do not lose their health, although they remain in that condition for many years. Provided that there is a pure air, and the water is of a wholesome quality, they do not even in any great degree fall away in their flesh.

In the present consideration of diet, it is only applied to bodily strength or muscular exertion, which is very distinct from courage: this last belongs to the brain and nerves as an emanation from the mind, and will be considered under the second division of this enquiry.

The strongest instance I ever met with of great bodily strength, not in itself producing courage, was in an Irishman who had been pressed into the navy as a sailor, and who was sent into the Naval Hospital at Plymouth. He spoke not one word of any known language, and therefore

was unfit for any duty, and was sent into the hospital to get rid of an incumbrance. Hunger and fear had the most powerful influence over him. He was put into an upper ward with some of his countrymen affected by the itch, and they disturbed the patients in the ward underneath by their unruly behaviour. Having a patient on whom I had performed an operation in the under ward, who had been deprived of rest by these riots, I went up to find out those who had been concerned in them. This man was pointed out. He fell on his knees, and held up his hands clasped like a child praying for mercy. He had the strength of Hercules, but no courage. When hungry, he would seize upon the mess of six men and carry off the whole allowance, none daring to oppose him, so great was his bodily strength. At the sight of a negro, no restraint could prevent him from making his escape, fear having got the better of every other consideration, not doubting that the object before him was in reality the devil.

SECT. II.

On the Diet best fitted to recruit the Energies of the Brain.

This is an enquiry the most interesting in philosophy, and one in which the physiologists could not engage, till our knowledge of the animal economy had arrived at its present state, and even now it cannot be pursued to any depth.

The brain and spinal marrow have offices assigned them, which would seem to be very distinct from each other, in so far as the one can be carried on in the best possible manner, under circumstances in which the other is very inefficiently performed.

One office is to dispose of the materials that have gone through the process of digestion, in building up and keeping the organs of the body in repair; the other, to receive intelligence by means of the organs of sense, and out of the various kinds of information thus collected form combinations which constitute the intellect.

There is no animal without a brain, and in no one is that organ so imperfect as to be unequal to the performance of the first of these offices. As the intellect must depend upon the number and perfection of the organs of sense, and the powers of the brain in making combinations from the information they supply, it is only met with in the human species in a state of complete developement: to man we shall therefore principally direct our present observations.

It is asserted that the degree of intellect belonging to an individual may be gathered from the countenance; this, however, is only true in part, for in many instances this is reversed; as men of deep research, whose eyes are turned inward to the mind, have more an appearance of stupidity in their countenance than other men: instances of this are Dr. Johnson, David Hume, and Simpson the professor of mathematics at Glasgow.

As the intellect, in my opinion, is in part derived from portions of the brain of each of the parents, partly from the state of the organs of sense, and much influenced by education, diet has nothing to do with its formation, but will do much for its support.

It is by means of proper diet that the judgment is rendered capable of continuing its exertions, and the mind is enabled to bear up against the daily annoyances that occur.

There is a very just remark upon this use of diet, in the History of England by David Hume. When speaking of the bigotry in the times of Mary, he says, "It may not appear difficult to check by a steady severity the first beginnings of controversy, but it renders men so delicate that they can never endure to hear of opposition, and will sometimes pay dearly for that false tranquillity in which they have been so long indulged; as healthful bodies are ruined by too nice a regimen, and are thereby rendered incapable of bearing the unavoidable incidents of human life."

When on the subject of sending supplies of blood to muscular fibres for their repairs, I mentioned that the blood could only be received in the relaxed state of these fibres,

and therefore the muscles that compose the left ventricle of the heart could only have supplies brought to them during the diastole of that organ. Something similar to this may be true with respect to the fibres composing the brain; and in that case these supplies can only be received during sleep or a state of their inaction.

A nobleman, while holding the high office of Lord Chancellor, in consequence of great morbid irritability, could not sleep: this state of watchfulness continued for fourteen days, and during that period he was unable to employ his thoughts to any rational purpose. When sleep returned this symptom went off.

I have already stated, that although the muscles and brain are both made up of parts of the blood, they do not both consist of the same ingredients: the muscles are principally composed of blood globules, and therefore it becomes an established fact that blood globules in great abundance in the food are essential to the support and repair of the muscular strength of the body.

It has been found, by experiments already detailed, that there is an entire difference between the ingredients of muscles and brain, and that the first are not decomposed by the act of freezing, while the other are. I have also learnt that the materials of the medullary substance of the brain are in such small proportion in the circulating blood, that when the blood is frozen and thawed, the loss of those parts from which the medullary substance is formed is so small, that the blood coagulates and goes through the same changes as if it had not been frozen.

The knowledge of these facts led me on to other

experiments on the brain, which I shall mention in this place. These were intended to ascertain whether all brains contain the same proportions of the transparent elastic jelly which is soluble in distilled water after death, and which is separated by the act of freezing from the globules it is employed to connect together during life, and drains off from the other parts; since it is not improbable if they do not, those brains that have the largest proportion of intellect may derive that advantage from this source.

It has been ascertained, that the proportion of this soluble substance in the human brain amounts to seventeen and a half per cent., since that is the loss produced by the act of freezing and draining off such parts as are dissolved. With this information, on the 18th of January, 1828, with the kind assistance of Mr. Farrady and my friend Mr. Walker, an old pupil of St. George's Hospital, I made at the Royal Institution the same experiment, which has been already detailed on the human brain, upon that of a sheep and a fox, as nearly as possible under the same circumstances. Both animals were killed at the same time, and a slice of the cortical and medullary part of the cerebrum above the lateral ventricles was taken from each brain, the one weighing 202 grains, the other 204 grains: this last was from the fox. They were separately wrapped up in tin-foil, and immersed in a freezing mixture, in which the mercury in a thermometer stood at zero. This was done as nearly to correspond in point of time after the animal's death, as the experiment on the man's brain had been made after death, which was about four hours.

At the end of four hours, the two portions of brain were taken out of the cold mixture, and the tin-foil coverings in which they were enclosed were unfolded, that the brain which was rendered completely solid might thaw gradually. The portions of tin-foil were not removed, but allowed to remain loose over them on an inclined plain to allow of drainage.

On the 20th, they were not completely thawed.

On the 21st, the portion of sheep's brain when examined was moist upon the upper surface, and there were drops of a pure liquid lying on the lower part of the tin-foil.

The fox's brain had no moisture upon its surface.

On the 23d of January, the sheep's brain had become more moist on the surface than it was on the 21st, but there was no moisture on the portion of the brain of the fox. As the parts had now become completely thawed, it was judged right to put an end to the experiment.

The parts were freed from all moisture, and reweighed on this, the fifth day.

The sheep's brain weighed 187½ grains.
The fox's brain weighed 199.

As it is fair to allow the loss from evaporation in five days to be two and a half per cent., the result will be a loss from moisture extracted by freezing,

In the sheep's brain six per cent.
In the fox's brain no perceptible quantity.

As no allowance was made in the experiment on the human brain for evaporation, it will be necessary before

we can bring it into this table to deduct two and a half per cent. on that account, which will leave the loss from freezing to have been seventeen and a half per cent.

This new fact of the human brain, and that of quadrupeds, although they have the same external form, and apparently the same internal structure, being different in the proportions of the materials of which they are composed, opens a wide field for physiological investigation.

It explains satisfactorily that the pre-eminence of intellect in mankind is dependent upon two causes; one, the quantity of medullary substance in proportion to the other parts of the brain being greater than in other animals; the other, there being also a greater proportion of the transparent elastic jelly soluble in distilled water, by which the globules that compose the fibres of the brain are held together, consequently these fibres perform their vibrations with a greater degree of accuracy as well as velocity.

Seeing the subject in this view, we can understand that insanity, in all its degrees, must be produced by the condensation of the medullary substance of the brain; as this is the only change of structure that I have ever met with in the examination of the brains of those who have been long deranged. We can also see the reason why insensibility immediately follows undue pressure on the brain, and goes off as soon as such pressure is removed. If this more delicate texture of brain is denied to animals who are doomed by their nature, and led by their natural instincts, as well as by the construction of their digestive organs, to live wholly on muscular flesh, we must conclude that the bodily strength which they derive from this diet is a

compensation for having a less degree of intellect than other animals who live entirely on vegetable substances.

The quadruped that has the largest proportion of brain when compared with its bulk is the elephant: it lives upon the most nutritious part of vegetable productions, farinaceous matter, and is provided with teeth capable of preparing it for the purposes of digestion in the most efficient manner. This, also, is the most sagacious, the most docile, and the most grateful of animals to those who treat it with kindness. As the elephant is the extreme link at one end of the chain in which is comprehended that part of the class mammalia that feed on plants, so the other, as far as regards the simplicity of its vegetable diet, is the xariffa, which is the mildest of brute creatures. We have sufficient evidence that vegetable diet in animals produces mildness in disposition, and animal food, ferocity. It is so with mankind in a greater degree than could have been previously imagined.

In the state of nature in which animals live, the only object of their diet is to support their bodily strength and get rid of the distress produced by hunger.

When quadrupeds and birds are placed by nature in pastures that are too luxuriant, so as to induce them to exceed in the quantity of their food beyond what health requires, this is corrected by the peculiarities of their digestive organs.

The cassiowary of Java, the most fertile part of the globe, has no power of retaining its food beyond the time necessary for health; but the ostrich of Africa, which has always a scanty fare, can retain it till there is enough to

supply the animal with nourishment, as has been formerly explained.

Man in these respects has no natural checks, and too often, when mixed in society, exceeds in the enjoyments of the table, and too often increases his appetite by the use of condiments, to his own detriment; and instead of strengthening his bodily health or his mental exertion, destroys both.

We must however admit, that condiments have great power in strengthening and in repairing the energies of the mind, and that the brain can be called into action by their means, even when it could not be done by food alone. A nobleman who was fond of music, could compose harmonious tunes for hours together under the influence of wine, but under no other circumstances.

Condiments are as necessary for long continuance of mental exertion, as flesh of quadrupeds is for continued feats of bodily strength; and it is a proper question to discuss in this place, whether such use of condiments is injurious to health, or tends to bring on premature old age? This I consider by no means to be the case, and, on the contrary, that they can prolong life.

We are every day seeing men of good natural abilities, who, from attention to their own notions of health, live very sparingly, at the same time making great mental exertion in professional pursuits, whether those of the law, or any other profession, and for want of the use of condiments, as wine to cheer them on their way, to keep them in good humour, and enable them to persevere in their exertions, they become a prey to diseases, and die at an early age, while others, who live more generously, go through the most

arduous mental toils, and retire from their duties at eighty or ninety without bodily infirmity.

There is no fatigue the human mind can undergo which is greater than that of applying itself with intensity, in quick succession, from one subject to another; and in doing this, the use of condiments, more than anything else, takes off the wear and tear that has been produced. Of this the nature of my profession has furnished me with sufficient evidence to make me satisfied of its truth. Nothing, however, can be more baneful to the human constitution than carrying too far this indulgence, and instances of this abuse are so common as to lead many individuals into the opposite extreme.

In warm climates, Europeans require a more generous diet for the preservation of health, under ordinary circumstances, to compensate for the exhaustion produced by heat, but the condiments employed are to be weaker than in colder regions. Alcohol and strong wines are pernicious; numbers die from drinking spirit and water, but not from the use of wine. The condiments should be principally spices, which may be indulged in to the full extent of the craving of the stomach.

The faculties of the mind, in warm climates, cannot make great exertions upon vegetable food alone, even with all the luxury that stimulating condiments enable the inhabitants to enjoy.

In Asia, it is understood that there is a diet more peculiarly fitted for exciting the venereal appetite; and this is so universally believed, that I can attach some credit to it. Two articles of this kind of food, the sea-worm, called bêche

de la mere, and the gelatinous nests of the Java swallow, form articles of trade between Java and China, and sell at a high price. They induce corpulency, and do not contain in their internal structure blood globules, but may have such ingredients as compose the rudiments of the brain and of the semen. We know from the deer, that the expenditure of fat during the rut is beyond all calculation, since, when the rut is over, there is no particle of fat to be found on the haunches of the male, which were well covered with it before the rut began.

Courage is an attribute of the mind over which diet, whatever strength it may give the body, can have little avail: it is in part the produce of education.

Sleep I have stated to be essentially necessary to admit of the repairs of the brain after great intellectual exertion; and when it is not resorted to for that purpose, the coronary arteries of the heart, and the branches of those of the pia mater, appear to become ossified, in which state they are unfit to convey the blood in sufficient quantity, and with the necessary impulse, to these organs.

What are the means of arriving at longevity, is a question that has been much agitated, and never can be completely answered, since we derive from our parents constitutions, if I may use the expression, which are fitted only to endure the fatigues of life for a certain number of years, and what that number is cannot be ascertained beforehand. Added to this, there must be freedom from disease, and I believe, in general, a certain mildness of disposition, and a certain portion of success in the progress through life, with a comfortable asylum in old age.

In Chelsea Hospital, which is an asylum for invalids worn out in the service of their country by military duty, during the six years I have been surgeon to the establishment, there have been several men above 100 years old. Mr. Hartshorn, who is my assistant, has been good enough to procure for me the following information respecting some of the pensioners, so far as it is connected with this subject.

Thomas Latter, aged 107, was brought up to be a weaver: hair and eyebrows always light coloured; has resided in this Hospital sixteen years, and enjoys good general health: his faculties are unimpaired.

He enlisted into the army in early life, was a considerable time in the West Indies, served in all the Duke of Wellington's campaigns on the Continent; reports himself as a man of temperate habits; has never been wounded, and never laboured under disease; and has had nine children by two wives.

Frederick Newstead, aged 105. His early life was passed in husbandry, till he enlisted as a soldier in the British army. During fifty-two years' service, he was present at the Battle of Minden; was several years in the West Indies and America. He never married, states himself sober and temperate in his habits; has been severely wounded in the right leg, but has enjoyed through life good general health, and has been a resident in Chelsea Hospital twenty-six years; has light hair and light eyes, his hearing and sight little impaired, and when his age is taken into the account is at present in sound health.

James Ball, aged 103, born in Longerinden in Bucking-

hamshire, was bred up to be a taylor, and enlisted in the army in 1745, in a regiment raising by Lord Harcourt. He served about thirty years; was in most of the campaigns in Germany; was never wounded; he married young, has outlived three wives and a son; he states his habits of life to have been temperate, and his health never impaired by disease. His personal appearance, if it is of age, is certainly a green old age; he is particularly intelligent; his sight much impaired, and he is rather hard of hearing; his hair and complexion, as well as his eyes, were dark; he has resided thirty years in the Hospital.

The diet of the establishment is animal food once in the day, potatoes, bread and cheese, and good malt liquor.

In all these instances, freedom from disease, and naturally good constitutions, have been the causes of the length of their several lives. In private life I have had many friends who have arrived at ninety years and upwards; they have gone through life without disease, and have been uniformly successful in their pursuits.

The materials of the human brain that are converted into water by the act of freezing, being so much greater than in quadrupeds, proves that in the composition of the brain of man, there is a larger proportion of oxygen, since water is principally made up of that gas. That the presence of oxygen will increase the elasticity of the fibres, is a self-evident proposition, and must render the brain more readily acted upon by impressions of every kind; nor can it be doubted, that vegetable food, from the water it contains, must bring into the stomach a larger supply of oxygen than it can derive from the flesh of quadrupeds, and the oxygen

that is conveyed directly to the circulation from the lungs, will be immediately carried to the brain by the carotid arteries. The difference in the animal spirits, which is immediately felt while breathing pure air, from what it is when the air is deprived of that state of salubrity, is in this way readily accounted for. Some individuals are much more sensible of this effect than others.

Two of the most nutritious substances belonging to the vegetable kingdom, are gum and sugar; and we find from their chemical analysis, that they consist of one half oxygen.

These remarks are confirmed by the experience of all ages, since it is agreed that pure air and pure water are necessary for the preservation of health, and requisite to the support of the energies of the human mind.

Bœotia was famous for the stupidity of the natives, and this was a bye-word among the Romans. The Dutch, in our days, are considered to be less lively, less intelligent than the other nations of Europe, in consequence of the fogs by which their climate is infested.

The ancient poets praised the water from the living spring, a draught of which was more exhilarating to the stomach than any other liquor. These advantages of a southern climate their poets knew how to prize, and there they cultivated with so much success the powers of the imagination, their food not being too gross, and allowing the materials of the brain to possess the degree of elasticity it requires.

It is in these climates we have at this day the improvisatori fed upon olives, macaroni, and iced water. Milton

studied in Italy, and there laid the plan of the Paradise Lost!

This is the diet and the climate for nourishing poets; yet in all ages they sing the praises of generous wine, which they enjoyed at the tables of the great where they sung their poems, and confess that it enabled them to tune their lyres to higher notes, and give the more sublime beauties of poetry to their compositions. Nor was it ever considered by them as hurtful to their health, more particularly as they advanced towards old age, when it was believed requisite to enable them to prolong their exertions.

Horace, who understood this subject better than any other man, both as a poet and as one who, by living in the best society at Rome, had opportunities of making his own observations with respect to the effects of diet upon the intellect, gives his opinion in an Ode to Apollo, and, in my judgment, a very correct one it is: —

> Me pascant olivæ
> Me cichorea, levesque malvæ.
> Frui paratis, et valido mihi,
> Latoë, dones.

Man, from his superior intellect, has not been called upon to confine himself to the vegetables as they grow in the field, nor to the water which nature supplies him with. He is capable of preparing, by his own ingenuity, both the one and the other, so as to make them more suitable to his palate; but, unfortunately for himself and his children, at the same time too often to sow the seeds of disease.

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ALPHABETICAL INDEX,

IN WHICH ARE INCLUDED

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